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The Journal of Parapsychology

GARDNER MURPHY and BERNARD F. RIESS, *Editors*
ERNEST TAVES, *Managing Editor*

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General Stechert

The Journal *of* Parapsychology

Volume 3 JUNE, 1939 Number 1

EDITORIAL

Towards the end of 1938 J. B. Rhine asked us to take over the *Journal of Parapsychology*. He wished to have time for his own research free from the interruptions attendant upon editing a journal. And in view of Dr. McDougall's death, he felt that the burden of the journal, if he continued alone, would be unbearable. We decided to accede to his request provided certain changes in the journal plan could be introduced. The most important of these changes are as follows: (1) The journal is to appear only twice a year, each number containing about 150 pages, including a long experimental monograph and a number of short papers. (2) Emphasis is to be upon consistent technical reporting with very detailed accounts of experimental and statistical method, but the general reader's needs will be met by prefacing each monographic report by a brief abstract.

We should have hesitated to undertake so large a task were it not for the generous assistance offered us by a committee of review set up after the September, 1938, meetings of the American Psychological Association. This committee came into existence as the result of Dr. Sells' suggestion at the ESP Symposium in Columbus that it would be advisable to present reports in a manner to permit psychologists to form a more definite judgment as to the adequacy of the experimental procedures used. Dr. Rhine asked that Dr. Sells act as chairman of such a committee of review and criticism of ESP papers, and a group of interested psychologists responded affirmatively to Dr. Sells' request that they serve upon such a committee. The present editors regarded the value of such critical reading of reports as huge indeed, and determined to send manuscripts submitted to this journal to Dr. Sells' committee for reading and critical comment. The policy of this journal will be to make the utmost possible use of the review committee, at the

same time holding neither the committee nor any individual member of the committee responsible for any shortcomings whatever either in the journal or in any single contribution to it. In fact, the journal can only proceed on the basis of a general rule current in scientific journals that the responsibility for facts and interpretations lies with authors and with them alone. This latter rule does not exempt the editors from responsibility in doing all that is possible to make certain that experimental conditions are actually as described and that results are actually as reported. But, as in all things human, the final responsibility lies with those who report what they have done in their laboratories.

The plan to publish one long monograph and a number of short articles in each number does not mean that long articles are regarded as complete or final statements of their problems nor that short articles are regarded as of less value than long ones. It seems important, however, to insist that ESP research be intensive and thorough, rather than extensive and superficial. In general, short articles are published for what they may be worth as suggestions in the direction of method, and will in all cases be followed, we dare hope, by technical and complete monographic reports.

Readers are very strongly urged to send comments on the present issue to the editors or to any member of the Board of Review* and to help us maintain a journal which will be of serious professional interest to psychologists, as well as being instructive to general readers. There is no reason for cultism or special pleading in the present type of investigation. Science in a democracy is the business of everyone, and efforts in scientific research are a fair target for anyone's criticism. We hope that all experiments reported here will be criticized and, even more important, repeated by impartial investigators.

G. M.
B. F. R.

* At present the members of the reviewing committee are: Dr. Saul B. Sells (Chairman), U. S. Works Progress Administration, 70 Columbus Ave., New York, N. Y.; Dr. Lillian Dick, Sarah Lawrence College, Bronxville, N. Y.; Dr. J. J. Gibson, Smith College, Northampton, Mass.; Dr. E. R. Hilgard, Stanford University, Stanford, California; Dr. J. L. Kennedy, Stanford University, Stanford, California; Dr. Irving Lorge, Teacher's College, Columbia University, New York, N. Y.; Dr. Louis Long, College of Physicians and Surgeons, New York, N. Y.; Dr. John Volkmann, Columbia University, New York, N. Y.; Dr. R. R. Willoughby, Brown University, Providence, R. I.

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REQUIREMENTS AND SUGGESTIONS FOR AN *ESP* TEST MACHINE

J. B. RHINE
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ABSTRACT: The general requirements for an adequate ESP test machine are described in considerable detail. General requirements demand that such a machine be extraordinarily stable, of simple design, not subject to tampering, and capable of rapid operation. More specifically, the machine should make mechanical selections, at random, of test material, and should provide ready means of recording results as the experiment proceeds. Other desirable, but not absolutely essential, conditions are described. Appended suggestions are made as to the requirements for a somewhat simpler machine than previously described.

In view of the great importance of developing a suitable ESP test machine and of the very considerable interest which has been raised in machines that are already in the course of development, it seems advisable to describe for the attention of those who may be interested the general requirements for an adequate ESP test machine, as we know them at present. The presentation of these requirements may enlist the interest of a number of persons who are equipped to offer valuable suggestions and, it is hoped, in many instances to invent and construct machines of their own design. If a considerable number of different designs are produced and a number of machines of different construction are developed, naturally we shall more quickly advance to the discovery of those best adapted to the purpose of the research.

Any description of the requirements for an ESP test machine is of necessity relative at this stage. Very probably the first attempt at the use of the machine will lead to modifications of requirements or extensions which will demand consideration. The valuable pioneer work of Tyrrell in developing an ESP test machine (see June, 1938, *Journal of Parapsychology*) has demonstrated the difficulty of anticipating requirements, and we may profit greatly by his experience.

Again, some of the requirements stated in this account are relative to certain uses intended and not absolute for all. The description taken as a whole is an ideal from our present point of view, toward which the designing engineer might conceivably aim. If there are some require-

ments which cannot well be met, we must, of course, adjust accordingly to the best obtainable product.

At the end will be found appended some suggestions for a less ambitious machine, suitable for certain tests of more preliminary character, a machine which is much simpler and less expensive to construct.

GENERAL CONSIDERATIONS

1. Because of the serious disturbance to the experiment and to the subject's morale which results from interruptions, it is highly important that the machine be thoroughly durable and dependable. More than ordinary stability is therefore required.

2. Simplicity in design is of concern for three reasons: (a) It will contribute to essential economy. High cost of such a machine would greatly limit its range of use. (b) Repairing—if it comes to that—would of course be easier the simpler the machine. (c) Presumably greater stability would go with minimizing of complexity.

3. Associated with the point of economy, it should be mentioned that the machine should be designed, even in the interests of research, to be marketable. Should selling price permit, not only are colleges and other research institutions possible purchasers, but wide public interest in the ESP research and the gamelike character of mechanized tests may develop a possible demand that will lead to discovery through amateur exploration as have certain other branches of science.

4. A machine for such purposes would have to be subject to easy check in order that frequent assurance could be obtained that it was performing its various functions properly.

5. The machine should be portable. It might consist of two units and probably would be better so.

6. It should be impossible to tamper with this machine so as to make it produce extra-chance results except by means of ESP.

7. The machine should be capable of rapid operation. One complete trial should be made in not more than two seconds and it would be desirable to have more speed, one trial per second for example, if obtainable.

8. If necessary to have the machine operate electrically it should be fitted to draw power from the local power circuit (60 AC, 110V).

SPECIAL REQUIREMENTS

1. Perhaps the main requirement is a mechanical selection system which will select one of five small compartments in which objects (ESP symbols will perhaps be commonly used) will be placed. The selector

will automatically turn on a light in the compartment selected. (Should a non-electrical machine be devised, some mechanical way of indicating the selection may be substituted for the lights; for example, a card may be raised or exposed within one of the compartments.) This selector must have a number of special features:

(a) No sensory cues must be given the subject. It is desirable to have the five compartments, the selector mechanism, and whatever recording device is used to register the selection made—all in a separate case which can be removed to a room distant from the subject. A push button on the panel in front of the subject is, of course, necessary to energize the selection system when desired. (In some experiments it may be desirable for the experimenter instead of the subject to press the selector switch, or even to have it automatically energized at a fixed rate. This will call for special adaptation.) A mechanical lever would do for a non-electrical system.

In case a selection mechanism could be devised which can either be safely regarded as soundless, or from which the sounds are of no indication value in identifying the box selected, there will, of course, be no need to remove the box containing the selector system. But this would require strong assurance of the adequacy of the exclusion of all cues, auditory and visual.

(b) Selections must represent random distribution. There must be no significant favoring of one compartment over the others in the total number of selections over a lengthy series (e.g., in 10,000 selections, no one box should be actually favored) nor must there be extra-chance distribution in, for example, avoidance of repetition or in excessive repetition.

(c) It should be possible to check the distribution of selections for a long series not only in advance of experimental use of the machine, but during the period of its use. This calls either for a system of individual counters for each compartment selected or for a permanent record of the selections made.

(d) The selector mechanism should be adequately safeguarded against possible tampering. It must not be possible to alter its wiring or recording so that its results will be unduly influenced. This may call for a system for locking the case containing it.

The selector mechanism apparently offers the greatest range of possibility to the inventor. In addition to the above limitations, there are the general considerations of expense, reliability, speed of operation, and others to be kept in mind.

2. The requirement next in importance is that of the recording of the results. Mechanical counters would offer one means of doing this. However, it would be highly desirable to have actual records of selections and of the subject's choice, preserving in the record the actual order in which these occurred. This would permit certain studies to be made that would be impossible on the mere record of the counters themselves. Some further details are in order:

(a) If counters alone are used, the following are needed: total trials made (1), total hits made (1), individual compartments selected (5), individual choices made by the subject (5), individual compartments selected *and* hit (5), number of trials in the run (i.e., up to 25), with counter resetting automatically at the end of the run (1), number of hits in a run—this counter running up to a possible 25 and resetting at zero when, after each run of 25, the starter button is pressed to reset the machine for a new run. This calls for a total of 19 counters, two of them of peculiar construction requiring to be reset automatically.

The trials-in-run and hits-in-run counters may be mounted on the panel before the subject in order to guide him in the course of the run. The individual choice counters (5) may be mounted behind the subject's panel for convenience. All others should be mounted in the remote box if two are provided. In any case, the total trials and total hits counters must be securely locked to safeguard against tampering. This should be done so as to permit the counters to be read through windows. It is necessary for all the counters (with the exception of the individual selection counters) to be accessible to the experimenter without having to unlock the apparatus. These should be visible only by unlocking the case unless they are wired to record only after the choice button is pressed and the trial over.

(b) If a recording system is used, the 15 individual counters may be eliminated. The other 4 would be required. In a machine of reduced cost, however, the total hits and total trials relays could, with some sacrifice of convenience, be eliminated in view of the availability of permanent records.

The recording system, too, must be locked and safeguarded beyond possible manipulation. The recording should be on a continuous roll which can be officially stamped at the beginning.

3. In addition to the general switch energizing the whole apparatus, there should be a switch reversing the usual order (mechanical selection first and subject's choice second) making it the routine for the subject

to press one of the choice buttons first and then to press the mechanical selection button. This permits the subject to register a prediction as to what he thinks the selected compartment will be. This pre-selection procedure, as we may call it, is designed to test the hypothesis of precognitive ESP. The apparatus will commonly be used to test ordinary ESP. (We may call this latter the post-selection procedure.) By means of a single switch, we should be able to throw the apparatus into use for post-selection on the one hand and on the other for pre-selection.

4. As soon as the selector button is pressed in the ordinary post-selection procedure, an indicator (let us say, green) is lighted on the subject's panel (as well, of course, as a bulb being lit in one of the five compartments). In the pre-selection procedure, this green light will come on when the subject makes his choice. The second step (pressing the selector button) will incidentally turn it off.

5. At the end of 25 trials, another light (e.g., red) will come on (a trial registers and is numbered from the choice buttons pressed in post-selection, and from the selector button for pre-selection). This light will appear on the completion of the twenty-fifth trial and will go out when the machine is reset by a special switch for a new run.

6. A reset switch is required to prepare the machine for the next run; that is, to turn off the red light indicating the end of the run and to reset the trials-in-the-run counter to zero.

7. As just mentioned, it should be made impossible to register or record more than one choice at a trial. This may be done by making it mechanically (or electrically) impossible to press two or more of the choice buttons per trial.

8. A switch should be installed on the subject's panel enabling him to cut off the recording system as well as the total hits and trials counters for "off the record" runs desirable for preliminary experiments.

CONDITIONS DESIRABLE BUT NOT ABSOLUTELY REQUIRED

1. A small inconspicuous white light could be installed on the subject's panel which would come on when the general switch to the apparatus turned on.

2. When the switch is pressed to reset the machine for another run, it would be very advantageous to have a small card released automatically stamped with the record of the number of hits made from the last run. This stamping and releasing would be energized by the closing of the reset switch. This is suggested by the public penny scales which

release a ticket with the weight printed on it. The advantage to the interest side of the research in having this device installed would be considerable, especially for child subjects.

3. It would be desirable to have neat cases holding the apparatus and an attractive panel for the subject. This panel might be in part built into the door of the apparatus case which, when open, reclines downward toward the subject making a small angle with the table. Thus the subject's main responses would involve easy downward pressure. The light, counter, and other switches could be on the upright panel in front of him.

4. It is desirable to have the telegraph key type of switch for the use of the subject because, first, they require little pressure because of good leverage and, second, there is less likelihood perhaps of incomplete operation of the multiple switch.

5. It is suggested that the best arrangement for the choice switches on the subject's panel is that of a semicircle. In the center of the circle the selection switch may be placed. It may be of advantage to have an additional selection switch which can be plugged in and moved from one side to the other in case the subject prefers to use his free hand on the selection switch, leaving one hand entirely free for the choice buttons. This extension would also allow the experimenter to do the selecting if desired. The radius of the semicircle and the circumference at which the choice buttons are spaced should be about three to four inches. The buttons should be large, about the size of the ordinary telegraph key, with a special button for the selection system.

APPENDED SUGGESTIONS FOR A SIMPLE MECHANICALLY OPERATED ESP APPARATUS

A less complicated apparatus might be found very satisfactory for certain limited test purposes in ESP work, and the greater simplicity and economy may encourage wider usage. The following suggestions are offered by way of a beginning for those readers who may be interested.

A. *General.* A non-electrical machine is suggested, enclosed in locked case with glass top through which a record of the last 25 actual target-and-call symbols may be observed as exposed on a continuous paper roll. This roll is the permanent record and the immediate check is made through the glass window.

B. *Stimuli.* The symbols are to be in five boxes, as above, and instead of lighting the box selected the card therein will be raised from an

inverted position to an upright angle facing the subject. The boxes are, of course, lightproof. Any symbols or cards may be used.

C. *Recording.* Recording on the continuous roll should be accomplished either by the typewriter principle (effected by the pressure of the choice keys) or by the adding machine plan of setting the type to be recorded by key pressure and actually energizing the recording by a lever operated by another hand. (The latter may be better since the mechanical selection has to be recorded also, and not before the choice of the subject is recorded. That is, the selection is made, but not recorded, and the recording apparatus is in readiness to record the selection. This is done simultaneously with the recording of the choice.)

D. *Selector.* There is needed a compact and reliable mechanical selector that will be free from extra-chance tendencies to favor any one symbol. This is likely the most difficult part of the machine because of the requirement of randomness.

The selector should be worked by an arm or lever that can be attached to either side of the machine. Pressing the arm makes a selection which raises a card in one of the five boxes to a vertical position, the face turned toward the subject. It remains there until a choice is made, the choice button releases it and it falls back into position. The selector arm also sets the recording device in position to record the selection made, this position being held until the choice button is pressed and the record of the selection made along with that of the choice itself. The entire machine should be returned to a state of readiness by the choice button.

E. *Regulation.* A run of 25 trials should be indicated by extra spacing in the record tape. This can be arranged by use of a wheel with 25 teeth running the spacer, and having a longer reach on the 25th.

F. *Safeguards.* (1) There must be no way of favoring selection of a given symbol or box by mechanical means, such as inserting an instrument, tipping the machine, or any other measure available to the subject without breaking open the case in which the machine is securely locked. (2) It must be impossible to gain any advantage by pressing more than one choice button at a time. (3) The selector must be soundless or make only indistinguishable sounds.

G. *Extras, desirable but unessential.* (1) If a bell can be sounded by the apparatus as the 25th trial is completed, it would be very advantageous. (2) If a lock arrangement can be added, holding a cover over the glass window during the run of 25 trials, and releasing the catch when the 25th is completed, this will (a) take the place of the

bell (the release of the cover calling attention to the ending of the run); (b) conceal the order of selections from view during the run and thus make the problem of the selector somewhat easier; and (c) avoid possible distraction from watching current hits and misses. The score for each run can be obtained, however, when the run is completed. The cover would have to be pressed down until it snapped fast before the next run could be started (let us say, the selector arm would not work with the cover up). (3) Mechanical counters of total trials and total hits could be inserted—also, of course, individual selection and choice and hit counters for each symbol and each choice button. But with a complete tape record only totals (trials and hits) would be of importance.

A MACHINE FOR RESEARCH IN EXTRA-SENSORY PERCEPTION

ERNEST TAVES

ABSTRACT: Though granting that hand-shuffling can properly be used for many ESP procedures, there are various situations in which automatic preparation of material to be used in ESP experiments is desirable. A machine has been devised which, by means of remote control, rotates so as to set up a set of 150 stimulus objects to be guessed.

The machine consists of a large tray divided into 150 small compartments—six rows of twenty-five. Each compartment can contain a disc numbered in one way on one side, in another way on the other side, or any regular solid polygon, such as a die. The tray is electrically rotated, and is brought to rest in a horizontal position, a photograph of the set-up being taken by remote control.

Preliminary data suggest that the dice and discs behave in a manner not normally predictable, and that the machine can properly be used for setting up a target normally known to no one.

FOR SOME TIME researchers in extra-sensory perception have realized that it would be of value to have stimulus objects prepared mechanically in a random manner. The purpose of this paper is to describe such a machine, construction of which has recently been completed by the writer.

In another paper[1] in this issue of the *Journal* is described considerable research using various types of cards as stimulus objects. The machine to be described here was constructed in such a manner that a comparable set-up of stimulus objects could be prepared without directly involving any human agency. That is, it was desired to construct a machine which would prepare six sets of twenty-five objects, the objects being such that the odds against their being "guessed" correctly were in some cases even, in some cases four to one. For the fifty-fifty odds, flat discs or chips may be used. In this case either face of the disc may be "up," and the subject is required to guess which face is actually uppermost, the disc lying flat. There may be a circle on one side of the disc and a cross on the other, for example.

For the situation in which the odds are longer dice may be used, in

[1] MURPHY, G. and TAVES, E., Covariance methods in the comparison of extra-sensory tasks, this issue, p. 38.

which case the subject is required to guess which of the six faces of the die is uppermost. This gives odds of five to one rather than of four to one as with the standard ESP cards, but this presents no serious difficulties. Other regular solid polygons could be used.

In first planning the machine, then, the materials decided upon were flat discs and dice. The next problem was concerned with the manner in which these materials were to be "shuffled." It is reasonable to assume, barring mechanical perfection, that if either a die or a flat disc is placed in a box, and the box is rotated for a short time, allowing the object inside to bounce freely from side to side, the face which will be uppermost when the box is stopped will be unpredictable. Acting upon this assumption a tray was constructed which met the necessary requirements.

The tray consisted simply of six rows of twenty-five compartments, the dimensions of which were 1¾" x 1¾" x 2½". The base of the tray was a 4' x 1½' rectangle of ¾" plywood, and the sides of the tray were constructed of the same material. The tray was divided into six longitudinal rows by partitions of wood, and each of the six rows was divided into twenty-five compartments with the use of thin veneer partitions, which were inserted in slots in the longitudinal partitions. A polished plate glass lid, 5/16" thick, was fastened over the tray with ¾" angle brass. This angle brass was, on one side of the tray, readily detachable, permitting easy removal and replacement of the lid.

The next problem was that concerned with rotating the tray longitudinally. This involved the question of rate of rotation. If this rate were too slow, the objects would simply slide from side to side, and would not be adequately bounced; if the rate were too fast, centrifugal force would hold the objects flat against the outermost side of the compartment. It was found that a rate of rotation between thirty and forty-five r.p.m. would meet the requirements. Thus a ratiomotor was used, which rotated the tray at thirty-eight r.p.m. The ratiomotor used, made by the Boston Gear Works, was simply a unit consisting of an electric motor and a gear reduction system. The motor was of the sixty-cycle, single-phase, ac type, operating on 120 volts.

The ratiomotor was mounted on a 1½' x 1½' plywood platform, and the platform was mounted on one end of a six-foot wooden base.

Shafts of ⅝" steel were attached to each end of the tray. One shaft ran through an adjustable bearing and a semi-flexible coupling to the ratiomotor, the other through a pillow block mounted on the base, opposite the motor platform. Thus the tray was rotated longitudinally.

Next came the problem of controlling the rotation adequately. Since operation was to be by remote control, it was necessary automatically to stop the tray in a horizontal position. One way of doing this would involve the insertion of a clutch between the motor and the tray, so that the tray could be disengaged, and brought into a horizontal position by a counterweight on the bottom of the tray. The clutch would be necessary since the gear reduction system, being of the worm type, did not permit reverse rotation; otherwise the tray, provided a counterweight were properly adjusted, would swing into a horizontal position immediately after the motor current were cut off.

The clutch method proved to be impracticable, and it was necessary to use other means. An electrical commutator was therefore used. Two brass discs, 4½" in diameter, were placed on the shaft between the motor and the tray. One of these discs was continuous, one discontinous. The two discs were so placed as to dip into open mercury cups, immediately below. One disc would then be in the mercury continuously, the other intermittently. If the mercury cups were then made the poles of an electrical circuit, current would pass through the circuit only when both discs were in the mercury. Thus the commutator would serve as a simple switch, either to start or stop the motor. This electrical system is shown in Figure 1.

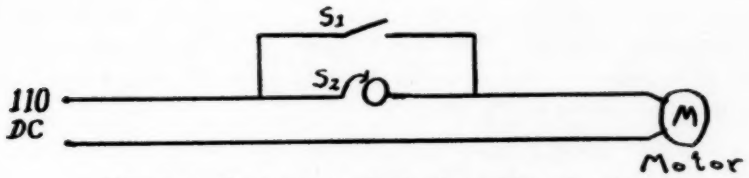

FIGURE 1. Electrical circuit showing wiring of motor.

S_1 is a switch operated by the experimenter in a distant room. S_2 is the commutator switch. When either switch is closed the motor will operate. The "break" section of the commutator was of an arc five degrees less than the arc through which the tray coasted after the current was cut from the motor. Thus when the experimenter's key was released the discontinuous commutator disc would be either in or out of the mercury cup. If it happened to be in, the motor would run until the disc left the mercury, breaking the circuit. If it happened to be out of the mercury the motor would coast until it dipped in again, would run until it dipped out, and would then coast to a stop as before. Or if the experimenter's key was released just as the discontinuous disc left the mercury the result would be the same. The five degree allowance

in the discontinuous disc was necessary to prevent the circuit from being continually opened and closed.

The assumption of the writer at this point was that the distance which the motor coasted after the current was cut off was constant. Thus assumption was not justified; the distance which the motor coasted was not constant, due to the varying viscosity of the lubricant in the gear reduction system as the motor gradually became warm. That is, the friction in the system was not constant. Therefore it was necessary to add to the system an additional, *constant,* friction of such magnitude as to blanket the variability previously present in the system.

For this purpose a flat pulley was placed on the shaft between the motor and the tray, and a leather belt was run over this pulley and down to the base, where it was rigidly attached. This additional friction lessened the distance which the tray coasted after the motor stopped, and also lessened the variability of this distance, so that the final stopping position of the tray was subject to a variability of less than ten degrees.

At this point a small empirical test was made, and it was found that although the tray stopped in the same position each time, the discs did not always lie flat on the bottom of the compartments; they sometimes would lean against the side of the compartment. To avoid this the commutator was adjusted so the tray stopped about fifteen degrees beyond the horizontal position. Also, the bottoms of the compartments were lined with celluloid film in order to reduce the friction between the discs and the bottom of the tray.

It was also found that the dice did not bounce from side to side, but simply slid from one side to the other. Thus it was necessary to place small pads of soft sponge rubber on one side of each of the compartments in which dice were to be used. These pads caused the dice to bounce freely and unpredictably.

In addition to preparing the stimulus objects by remote control, it was desirable to record the entire set-up in the same way, so that no person would have to be in the room with the tray for any reason whatever. For this purpose it was necessary to use a camera which would, in a single operation, expose the film and move the film into position for the next exposure. The Sept camera, of obsolete French make, was found to be adequate for this purpose. This is a movie camera which can be used on a single exposure basis.

The camera was mounted above the tray, the plunger being operated by a heavy solenoid, controlled from a distant room. One loading of

the camera permits as many as 250 exposures. It is thus possible for an experimenter to go through 250 experiments, each consisting of 150 trials, without entering the laboratory room in which the apparatus is placed.

Illumination for the photography of the set-up was provided by two No. 2 Mazda Photoflood lamps in reflectors, mounted above the tray. A sketch of the entire apparatus is shown in Figure 2.

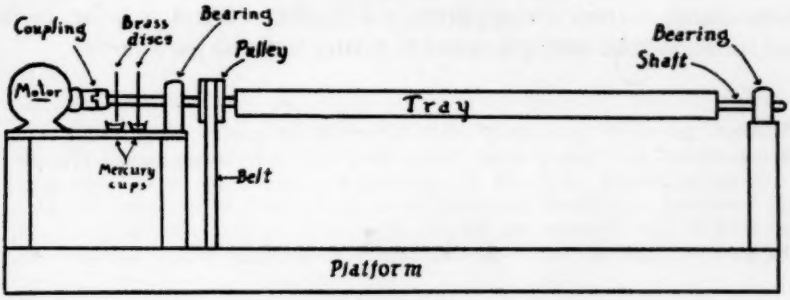

FIGURE 2.

The photographic aspect of the recording is desirable for several reasons. For experiments to be conducted on the sole responsibility of the experimenter and his immediate assistants, the negatives need be used only, a projector permitting easy reading of the impressions. And for co-operative experiments in which other members of his own university staff may watch from beginning to end every phase of the experimental work, examining the results at leisure afterwards, and for inter-university long-distance work involving the exchange of records by various experimenters, reliance must be placed upon photographic prints. Also, the photographic field can include an electric clock and a calendar card, and the postmark of mailed photographic prints can show the last moment at which a particular experiment could have been performed.

One of the advantages of the machine described is that a great variety of materials can easily be used, it being necessary only to remove the glass lid of the turntable to effect immediate change of test objects. It is possible, for example, to use material of emotional quality. Discs may be used, on one side of which is a simple drawing, such as a tragic profile, there being on the other side of the disc a comic profile. Various types of colored discs may, of course, be used, as well as great variety of types of symbolic material.

Another advantage is that the machine provides a means for self-testing. While the laboratory in which the apparatus is placed remains locked, the subject can set up his own targets by remote control without necessitating the presence of an experimenter. The records of the subject's guesses can be placed in the hands of the person responsible for the experiment before anyone is admitted to the laboratory in which is placed the machine.

An empirical control of some length has already been made, and no extra-chance factors are apparent. A further control is being made, and results will be fully presented in a later issue of the *Journal*.

REPLY TO CRITICS OF THE STANFORD EXPERIMENTS ON THOUGHT-TRANSFERENCE

JOHN E. COOVER[1]
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ABSTRACT: Critics of Coover's 1917 monograph, reporting research on thought transference (Stanford University), have stated that conclusions other than those drawn by Coover are indicated by the data. Coover points out that these criticisms are not valid because of fallacious selection of particular experiments, uncontrolled sources of error in the original research, and lack of regard for negative results obtained in experiments in which the subjects were of the "gifted" type.

IN THE MONOGRAPH[2] that has evoked protests from capable critics there are several sections that relate to the problem of thought-transference: (1) 1000 trials with one subject on the guessing of lotto block numbers, (2) 10,000 trials with normal subjects and 1000 with "sensitives" on the guessing of 40 playing cards, (3) 2400 trials on "the feeling of being stared at," (4) 15,000 trials on the influence of subliminal visual and auditory cues upon guessing, (5) data on mental habits, which play a part in "telepathy" because of community of experience, shown in guessing playing cards and numbers, in reading scientific instruments and in estimating magnitudes, (6) an exhibit of data on inductive probability showing safety of reliance on the theory of probability in evaluating the experiments, (7) a section on infinitesimal probability showing that highly improbable events may be expected to occur in daily life, and (8) a critique of the evidence offered to prove the reality of telepathy. The conclusions were in general negative with respect to thought-transference, but positive with respect to

[1] Due to the author's untimely death, this manuscript was left unfinished. I have added only the last part of the analysis of Thouless' criticisms and the concluding statements. The responsibility for these statements rests solely with me, although we were in essential agreement on them.—John L. Kennedy, Fellow in Psychical Research.

[2] COOVER, JOHN E. "Experiments in Psychical Research at Stanford University" Stanford University Press, 1917, xxiv + 641. (Now out of print.)

the "subjective" experience of telepathy in the laboratory, to the effect of subliminal cues in guessing, to the effect of mental habits and to the trustworthiness of theoretical probability in the evaluation of card guessing experiments.

The criticisms which I shall endeavor to answer apply to the section of experiments with 100 normal subjects on the guessing of 40 playing cards. These criticisms are based upon recomputations of results abstracted from the tables of data exhibited in the monograph, and tend to controvert the negative conclusions reached in the monograph.

Dr. F. C. S. Schiller, in his review[3] of the monograph, says of the card-guessing experiments,

> The experiments constituted a good test of the hypothesis that the capacity for receiving telepathic impressions is faintly diffused throughout the human race (p. 262).

He examined the final totals of the tables:

	Number Right on				Guesses
	Card	Color	Number	Suit	
Obtained					
Card-not-Imaged	141	2491	488	1252	4865
Card-Imaged	153	2556	538	1344	5135
Expected by Chance					
Card-not-Imaged	122	2433	487	1216	4865
Card-Imaged	128	2568	514	1284	5135
Deviation					
Card-not-Imaged	19	58	1	36	....
Card-Imaged	25	11	24	60	....
Chance Limit	47	150	90	130	5000

and observes,

> The results taken in bulk were negative in the aggregate, and so did not support the belief in faint degrees of telepathy. . . . There was a slight excess over the most probable number in both the Card-not-Imaged and Card-Imaged series, and a slight difference in favor of the Card-Imaged experiments in which telepathy might conceivably be operative; but it was not sufficient to warrant any inference as to the reality of telepathy, as it was not above the limits of chance variation.

And he notices that if the totals of the separate thousands of guesses are examined, some of them in the Card-not-Imaged series

[3] *Proceedings of the Society for Psychical Research* (London), 1918, **30**, 261-273.

might have been taken as better proof of telepathy than any in the Card-Imaged experiments (pp. 262-3).

Thus far the critic is in agreement with the text.

When, however, we come to test the hypothesis that telepathy occurs in certain *(rare)* subjects, the interpretation of these figures is by no means as clear. For it then becomes legitimate to argue that *some* only of the reagents had faint telepathic capacity, and to select their answers for separate analysis. . . . Hence we may select 14 reagents . . . and tabulate their results.

The totals of this tabulation of 14 sets of Card-Imaged experiments and of another tabulation of an equal number of selected results from the Card-not-Imaged series, are as follows:

	Guesses Right on		Guesses
	Card	Number	
Card-Imaged	54	119	711
Card-not-Imaged	49	91	690
Card-Imaged	7.6%	16.7%	
Card-not-Imaged	7.1%	13.2%	
Chance Limit	5.0%	14.8%	

Concerning the Card-Imaged successes, Schiller says,

There was therefore a considerable excess over the ordinary chance distribution. But was it too great to be ascribed to chance, and must it be attributed to an unknown cause ("telepathy")?

He refers to the table of percentages and learns that the per cents right exceed the chance limit, and continues,

This is on the face of it a considerable excess, and if the method of reasoning is sound, would appear to prove some degree of telepathy in the 14 selected subjects (p. 263).

It should be noted that there appears to be some question in Dr. Schiller's mind as to whether this procedure of recomputation is sound.

Dr. Schiller then turns his attention to the Card-not-Imaged results of the 14 subjects who got three or more cards right, and notes that on the "card right" his computation shows them also to exceed the chance limit, and on number right, closely approach that limit. In fact, there is little extra-chance cause to be granted the Card-Imaged group that cannot also be granted the Card-not-Imaged group. Does it seem to be telepathy or clairvoyance that may be the extra-chance cause?

Dr. Schiller notices that in the two groups of 14 best guessers five subjects are common and he computes their results separately:

	Guesses Right on		Guesses
	Card	Number	
Card-Imaged	19	45	240
Card-not-Imaged	22	41	260
Card-Imaged	7.9%	18.7%	...
Card-not-Imaged	8.5%	15.8%	...
Combined	8.2%	17.2%	500
Chance Limit	5.5%	15.7%	

The percentages . . . are very markedly higher than the maxima attributable to chance. . . . These figures therefore distinctly point to some source of rightness beyond chance in these cases. As it occurs in the Card-not-Imaged series, it cannot be of the nature of conscious Thought-Transference. But may it not be due to a sort of "lucidity" or clairvoyance in the subject? (p. 264).

Dr. Coover is hardly entitled to deduce from his data that "no trace of an objective thought-transference is found as a capacity enjoyed in perceptible measure by *any* of the individual normal subjects" (p. 124). He should recognize not only, as he does (Coover, p. 65), that his "control" (Card-not-Imaged) experiments assumed the non-existence of any "lucidity," but also that this may have been *wrong*, and that they may not have been "pure guesses," and endeavor to experiment further with the abnormal coincidence of good guessers successful in both series (p. 265).

If the excess of right answers continues (with these subjects), he will be able to establish the existence of telepathy on a statistical basis. Experiments with these five good normal subjects would appear on the evidence to be more promising than with the 10 "sensitive," whose 530 Card-Imaged experiments only showed a comparatively slight excess over probability, and whose guesses in Card-not-Imaged experiments gave no hint of lucidity (p. 265).

(Is it not curious that these selected normal subjects should show more promise for lucidity than do mediums and "sensitives" who are reputed to have it?)

In a footnote (p. 264), Schiller draws attention to the fact that while in the two groups of 14 best guessers, five subjects are common to both sets, in two groups of worst guessers, 24 subjects in the Card-not-Imaged experiments and 20 in the Card-Imaged experiments, only three subjects are common to the two groups. The best guessers tend more than the worst guessers to express their trait in both the Card-Imaged

and Card-not-Imaged experiments. The best guessers showed results above chance expectancy in both groups; the worst guessers in the Card-Imaged experiments showed about chance results in their Card-not-Imaged experiments, but the worst guessers in the Card-not-Imaged experiments showed above-chance expectancy in their Card-Imaged experiments. Thus this last group of worst guessers shares with the best guessers some benefit from imaging the card.

We may carry further this method of recomputation of the results of selected groups:

1. There were 20 worst guessers in the Card-Imaged experiments, who, in sets of approximately 50 guesses, got zero cards right. (This was the basis of their selection.) Total number of trials is 1026.

Card right	= 0	n	= 1026
np expected	= 26	p	= .025
Difference	= 26	q	= .975
D/σ	= 5.12	σ^2	= 25.0
Chance limit	= 4.24		

Here the difference divided by the standard deviation (σ) of the binomial distribution is 5.12, which exceeds the adopted chance limit of 4.24. Is this proof of reversed telepathy? If the deviation is below the chance limit, the guesser must have known the card in order to miss it.

2. There were 24 worst guessers in the Card-not-Imaged experiments:

Card right	= 0	n	= 1143
np expected	= 28.6	p	= .025
Difference	= 28.6	q	= .975
D/σ	= 5.41	σ^2	= 27.9
Chance limit	= 4.24		

Again a result which is over the chance limit. Is this reversed clairvoyance?

3. Among a considerable number of sets of drawn cards matched with the *preceding* guess there were seven sets in which three or more cards were right. By selecting these for independent treatment we have the tabulated computation from p (probability of a card right) = .025, n (number of guesses) = 374.

Card right	= 24
np expected	= 9.35
Difference	= 14.65
D/σ	= 4.85
Chance limit	= 4.24
σ^2	= 9.12

This extra-chance result is significant because it exceeds the limit of chance fluctuation, set high for logical reasons and for convenience at 4.24 times the standard deviation. If the separateness of computation is sound, we seem to have proof of prescience, prediction, or prophecy. The relation is between the guess and the following drawing of a card from a newly shuffled pack.

4. A still more startling result may be shown from the separate treatment of the figures obtained in matching the cards drawn in one set of approximately 50 with those drawn in another; in four such sets there were 4 cards right in each set.

Card right	= 16	n = 197
np expected	= 4.93	p = .025
Difference	= 11.07	q = .975
D/σ	= 5.03	
Chance limit	= 4.24	
σ^2	= 4.80	

Were this extra-chance result to be warranted, we should have to search for some demon that correlates drawings of cards from immediately shuffled packs manipulated by different persons at different places and times or for mischance in tabulating figures in tables or in marking or counting coincidences; either demon, or (2) experimental, (3) clerical, (4) statistical, or (5) computational error. (I have numbered these alternatives because I believe them to be often neglected when they are potent for accumulating spurious coincidences.)

5. Suppose we now test this method of recomputing results of a group selected from a distribution by selecting for separate analysis only the results of the 8 subjects who got 3 cards right in the Card-Imaged experiments.

n = 399

Card right	= 24	Number right	= 67
np expected	= 10	np expected	= 40
Difference	= 14	Difference	= 27
D/σ	= 4.50	D/σ	= 4.50
Chance limit	= 4.24	Chance limit	= 4.24
σ^2	= 9.73	σ^2	= 35.90

In this last sample, the number of cards right and of number right is also above the limit of chance yet the probability of getting 3 cards right is .093 (the fourth term in the expansion of $(q + p)^{50}$) and .093 $\times$ 100 (sets of 50 guesses) = 9.3. On the basis of pure chance, 9 cases of 3 cards right are expected in the distribution. In our distribution of actual results in the Card-Imaged experiments we obtained

8 such cases (one less than chance expectation), but when we select those cases and recompute them separately, we seem to have statistically significant proof of an extra-chance cause.

It is evident that the separate treatment of a part of a distribution calls for a special method. Case 5 shows that part of a chance distribution may be made to show cause apart from chance. All one needs to do is to select sufficient data removed well from the mean of the distribution and he can "prove" the presence of any cause he has an interest in promoting.

Selection for separate treatment should not be made from the figures in the distribution; it should be made from the conditions of the experiment. Or, if selection is made on the basis of figures in the distribution, it should be for the purpose of going back to the data to seek for conditions that may be responsible for the peculiar nature of these figures, rather than for putting them through a statistical mill that is no more applicable to them.

This demonstration of the fallacy of "selected" cases in statistical populations is made for the benefit of those who not only overemphasize but misinterpret Dr. Schiller's criticism. Dr. Schiller, at the beginning of his consideration of the hypothesis that telepathy occurs only in certain rare persons, says,

> It then becomes legitimate to argue that some only of the subjects had faint telepathic capacity, and to select their answers for separate analysis (p. 263).

And, after exhibiting that the results of his recomputations were above the chance limits, he says,

> This is on the face of it a considerable excess, and *if the method of reasoning is sound, would appear to prove* some degree of telepathy in the 14 selected subjects (p. 263).

He notices that "card right" by the best 14 guessers in the Card-not-Imaged experiments is also above the "chance limit," and then turns his attention to the 5 subjects common to the two groups; after combining their Card-not-Imaged and Card-Imaged scores, he finds them "markedly higher than the maxima attributable to chance," and says these figures therefore *distinctly point to* (not definitely prove) some source of rightness beyond chance in these cases.

> May it not be a sort of lucidity in the subject? Doctor Coover is hardly entitled to deduce from his data that "no trace of an objective thought-transference is found as a capacity enjoyed in perceptible measure in *any* of the individual normal subjects" (p. 124). He should recognize not only,

as he does (Coover, p. 65), that his control "Card-not-Imaged" experiments assumed the nonexistence of any lucidity, but also that this may have been *wrong*, and that they may not have been pure guesses.

He does not say that there appear to be excesses here beyond chance guessing and that further experimenting with the best guessers, if they hold up, would "establish the existence of telepathy on a statistical basis" (p. 265), and might be more profitable than further work with sensitives in search of lucidity.

So far, then, (1) Schiller does not demur at the high critical ratio ($D/\sigma = 4.24$) chosen for the limit of chance variation. (2) He agrees that the totals of all the experiments do not pass this limit and that therefore the hypothesis that telepathy as a capacity existing faintly in all men is not supported; he draws attention to excesses beyond chance expectation in the results of selected best guessers that "distinctly point" to some source of rightness beyond chance in these cases. (3) He thinks the experimenter was "hardly entitled" to say that no trace of an objective thought-transference as a capacity enjoyed in perceptible measure by any of the individual normal subjects was found, and (4) he thinks that guessing in the Card-not-Imaged experiments was not pure guessing.

The monograph conclusions were: (1) No telepathy as a common capacity was found because the general totals did not exceed the chosen chance limit; (2) No telepathy as a capacity of single individuals was found because no totals of single sets exceeded the limit of chance. The same conclusions could have been deduced with reference to lucidity. After Schiller's criticism, both of these conclusions still stand. However, something more could have been done with the excesses that challenged Schiller's attention; these are sub-critical, below the limit of chance variation; while they count zero for telepathy, which is required to pass the limit of chance, they may count significantly toward establishing any known causes. But these may wait till their entrance in the next review to be examined.

Dr. Robert H. Thouless, of the Department of Psychology at Glasgow University, in his review[4] of Professor Rhine's monograph[5] takes note that Rhine quotes, among other investigations,

that of Coover as providing positive evidence of extra-sensory perception. Since it is a common opinion that Coover's results were entirely negative and show nothing but chance distribution, I have thought it worth while

[4] Dr. Rhine's recent experiments on telepathy and clairvoyance and a reconsideration of J. E. Coover's conclusions on telepathy. *Proc. Soc. Psychical Res.*, **43**, 24-37, 1935.

[5] "Extra-Sensory Perception," *Boston Soc. Psychical Res.*, 1934, pp. 169.

to re-examine Coover's figures and will discuss these before proceeding with Rhine's own work. Rhine is undoubtedly right in saying that Coover's results actually show strong evidence against chance. There seem to have been two reasons why Coover himself drew the opposite conclusion: first, he adopted an absurdly high limit for the deviation from mean expectation which might be attributed to chance, and, secondly, he did not consider the possibility that clairvoyance might be active where telepathy was impossible in the Card-not-Imaged series[6] (p. 25).

Dr. Thouless also inspected the general table presented first above, and stated that

The general tendency of both of these Card-not-Imaged and Card-Imaged series is clearly to exceed mean chance expectation, and in approximately equal amounts. Coover concludes that since the factor of telepathy cannot be present in the first series, the approximate equality of the two groups is due to the fact that the deviations of both are due to chance. A safer conclusion would seem to be that if any factors are present causing deviation from expectation, these are operating in approximately equal amounts in the two conditions of experimentation. At any rate, we shall be justified in lumping the two groups together for statistical consideration. For the remainder of the discussion of these results, I shall do this since it will give us the advantage of the higher significance to be obtained by larger numbers (p. 26).[7]

First, let us consider the reasons for adopting the "absurdly high" limit of chance expectancy. In a footnote on page 85 of Coover's monograph, we find the following explanation:

Although there are certain restrictions to the application of this formula, such as when n (the number of experiments) is small, or when p is very small, its use is peculiarly applicable to the type of data with which we are dealing, and is not unsupported by approved statistical methods already used in the field of psychical research. E., in his "Analysis of Mrs. Verrall's Card Experiments" (Proc. Soc. Psychic. Res., 1895, 11, 193-197). Mr. C. P. Sanger used the following formula for the limit of chance deviation from the probable number of occurrences:

$$K = 3\sqrt{2} \cdot (1\text{-}q)mq$$

in which q is probability of occurrences and m is number of experiments.

By appropriate transformations of symbols, this formula reduces to the

$$L = \sqrt{2} \cdot 3\sigma$$

used by Coover to evaluate his card guessing results.

Another hint as to the reasons behind Coover's selection of a high limit of chance variation is contained in a letter written to Mr. Whately Carington in 1938. To quote:

[6] See Coover (p. 65) for a discussion of assumptions in the Card-not-Imaged series.

[7] Coover's manuscript ended here.

It is agreed that if there is awareness of objective facts or events, without the functions of the organs of sense, it is of great importance to establish that phenomenon scientifically (i.e., by experiment). Also agreed that no negative judgment is justified now or ever. But what seems not to be agreed is that the history of the experimental work on the problem suggests that there is a very *small* probability that the hypothesis is true, and that therefore a *high* critical ratio for the limit of chance deviation is a logical necessity.

Professor Thouless does not seem to reject this logical assumption as necessary proof of telepathic transmission. He does object to Coover's conclusion that the results were explainable on the basis of chance variation. He says,

The difference between observation and mean chance expectation of the number of cards guessed altogether right by Coover's subjects shows a probability of chance occurrence very much below this limit (50-1. D/σ 2.05), being 200-1 (D/σ 2.58). The existence of some factor favoring correct guessing of the card is strongly indicated. It might be objected that any form of extra-sensory cognition is *a priori* so improbable that we shall be right to insist on a much more severe criterion of significance than we should need, let us say, if we were trying to investigate the difference in fertility of manured and unmanured fields. To this objection there are two replies. First, the question at issue is not, at the moment, whether or not extra-sensory cognition occurred amongst Coover's subjects but whether there was a factor in his experiments favoring correct guessing (such a factor might be some unnoticed error of method). There seem to be no grounds for regarding the presence of such a factor as very improbable. A much more important consideration, however, is that if we are convinced of the *a priori* improbability of extra-sensory cognition, that will be a sound reason for accepting the indications of a 200-1 odds against chance with less conviction than we should otherwise feel; it is no evidence at all for regarding heavy odds against chance as evidence in favor of the operation of chance (p. 27).

The cogency of this criticism is recognized. It remains, however, to return to Coover's description of method in order to evaluate the 200-1 odds. The control of these experiments may be questioned on two points, (1) independence of recording and (2) use of minimal and subliminal auditory cues.

In order to eliminate completely errors in recording at the time of the experiment which are due to knowledge of the correctness of the guess, the recording of the cards and the subject's guesses should be done separately and independently. Coover states:

The experimenter with a watch before him, (1) shuffles the deck of forty playing cards (the face cards being discarded), cuts the pack, and holds

the cards concealed; (2) shakes the dice-box, to determine a control or regular experiment, and, if the latter, the form of content the latter is to have in his mind; (3) if a regular experiment, he turns over the pack, exposing to his view the under card, taps once to signal the reagent that the experimental period begins, holds mental content of card and wills the content to be projected into the mind of the reagent, and, after 15 to 20 or more seconds, taps twice to signal the close of the interval. *After he notes that the reagent has recorded his guess,*[8] and has turned to his introspections, he records the color, number and suit of the card and the number of the die-spot which conditioned the form of the experiment (as, R5H 1, for Red, Five of Hearts, Die-spot 1—i.e., precisely the same form as the regular, except that the card remained unknown until the reagent had recorded his guess (pp. 53-54).

These conditions for recording would have been completely controlled if the agent could not have seen the subject while the latter was recording his guess.

Referring back to the quotation on method given above, it is apparent that the experiments were carried out in the same room. Although 847 trials were collected with the agent at distances of 1, 2, 3, 4.6, 6, and 10 meters from the percipient, no differential effect to distance was discovered in the analysis of this series. It is possible, however, that occasional involuntary auditory cues may have been operative in producing small excesses over chance expectancy when the results of all the 5,135 Card-Imaged trials were summed. Coover (pp. 452-460) describes methods and exhibits negative results for telepathy or any other factor besides chance variation when conditions of the experiment were such as to exclude sources of error. These experiments were carried out with "sensitives" who claimed to have the ability to obtain information by telepathy.

The following conclusions may be drawn from this discussion:

(1) Critics of Coover's 1917 monograph have confined their attention to the card guessing series with normal subjects, attempting to show that selected subjects from this group of 100 gave evidence of ability to produce extra-chance scores. In his discussion of Schiller's criticisms, Coover has demonstrated the statistical fallacy in such selection.

(2) The critics have not given due regard to the negative results in his tests, using 10 professional and private "psychics," of the hypothesis that only a few selected cases have telepathic ability.

[8] Italics mine. J. L. K.

(3) Uncontrolled sources of error may explain the positive deviations on whole card right in the "normal series" of card guessing results. Coover's experimental conditions did not completely eliminate the possibility of errors in recording and involuntary auditory cues.

(4) These considerations do not support statements concerning Coover's "positive" findings for telepathy and clairvoyance which are appearing in the modern literature of "Extra-Sensory Perception."

ADEQUATE EXPERIMENTAL TESTING OF THE HYPOTHESIS OF "EXTRA-SENSORY PERCEPTION" BASED ON CARD-SORTING

ABSTRACT: Some psychologists reject the conclusions of Rhine and other authors favorable to the hypothesis of extra-sensory perception, since their conclusions have been drawn from data without full description of the conditions under which the data were obtained. A program of definitive nature is described. Suggestions are given regarding general procedure, safeguards against illicit cues, the way in which cards are handled by subjects, scoring and keeping of records, quantitative specifications, selection and orientation of subjects, selection of assistants, general superintendence of the experiment, protection of materials and records, and elaboration of obtained data.

DR. J. B. RHINE and certain other authors have drawn, from data derived from card-sorting, conclusions which are favorable to the hypothesis of "extra-sensory perception." Many psychologists have rejected these conclusions, alleging that the data, as presented, do not warrant the conclusions drawn, and pointing out the principle, generally accepted by experimental psychologists, that valid conclusions cannot be drawn from data in isolation, but can be drawn only from data in conjunction with the conditions under which the data have been obtained. As a result of these challenges, there has come from several quarters a request for the outlining of a program of research which would, if carried out, be acceptable to Dr. Rhine's critics, and from which might be expected results of a definitive nature.

In presenting the program which follows, we are attempting merely to apply to a specific problem the procedures, methods, and techniques which are familiar to, and accepted by, psychologists trained in the psychological laboratory, and accustomed to the requirements of rigid experimental work.

It must be emphasized that in no program is it possible, in advance of operations under the program, to cover all precautions, or to meet all contingencies which may arise, and therefore the provisions of the several sections of this research program may not be sufficient. It would, for example, be impracticable in the present outline to prescribe, for all possible situations, ways and means of excluding sensory clues

or cues, since the range of possibilities is large. It is necessary, therefore, that there be the most competent possible supervision, as indicated in Section IX below.

It must further be emphasized that the various stringent precautions prescribed in the several sections of the program involve no implications that previous experimenters failed to take such precautions as were deemed by them reasonable and proper, or that there was any lack of good faith on the parts of experimenters or subjects. The techniques and precautions herein specified are presented solely with a view to the preclusion, in so far as may be possible, of all subsequent criticism.

The experimental specifications here presented apply to a very specific problem, and obviously cannot be transferred, *simpliciter,* to a different problem. It is possible to furnish, within a small number of pages, specifications for a limited problem. To furnish specifications applying to all problems in the field would involve the writing of a manual on experimental psychology.

The problem has been limited in such a manner as to make the procedure involved correspond, as nearly as possible, with the procedure which appears to be the most widely employed by those who have made card tests of alleged "extra-sensory perception." The limitations of primary importance are: (1) as regards basic materials; to the use of the cards commercially sold as "ESP cards," or cards essentially similar in design and construction. (2) as regards method; to the sorting of the cards in the attempt to distribute them in stacks corresponding to the symbols borne on the faces of the cards. (3) as regards plan; to the deriving, for final comparison, of average scores of success or hits. (4) as regards the element of chance; to the condition under which the subject, at any time during a test, knows what disposition he has made of cards up to that point.

We may point out, that for experimental tests (1) with other materials; (2) with naming of cards instead of sorting; (3) with purposes other than the securing of average scores; or (4) in which the subject, in assigning a card, has no knowledge (of normal sort) of the disposition made of preceding cards; the specifications below will not apply without intelligent revision to fit the altered experiment. Many of the problems thus excluded are more entertaining, and undoubtedly better suited to illuminating the total "extra-sensory" question than is the simple card-sorting experiment here described. The controversies,

however, have centered around approximations to this experiment, and it is therefore the one with which we here deal.

I. *General Procedure.*

The method of ***simple matching*** involves: (1) thorough shuffling of the deck of cards, and (2) distribution of the cards to five stacks, each visibly marked with one of the five symbols borne by the cards. This distribution is made with attention to the possibility of assigning the cards to the stacks corresponding to the symbols borne by the several cards, and, presumably, with the desire to place each card in the "right" place.

In casual tests, the subject is prevented from seeing the faces of the cards during the shuffle and "deal," but is permitted to see the backs of the cards, and may even handle the cards, himself shuffling and dealing, and may record his own scores.

Such conditions are not satisfactory in an experimental test. The backs of the ESP cards offer both visual and tactual cues to the face designs. Although, theoretically, these cues could be obviated by a carefully planned lighting system and by requiring a certain technique in handling the cards, other forms of safeguard are more certain, and less productive of doubts on the parts of critics.

II. *Safeguards against Illicit Cues.*

1. The subject must not be permitted to see the cards, faces or backs at any time during the tests. This period of concealment, as a minimum, covers the time from the beginning of a shuffle to the completion of the record of the distribution. The soundest method of assuring the fulfillment of this condition is to provide, by proper screening, that the cards employed in the test shall *at no time* be seen by the subject.

2. The cards must not at any time be handled by the subject. All shuffling and dealing is to be done by the assistant, or assistants, or by one of the supervisors of the experiment. This restriction, and the one preceding, might be considered needless if ordinary playing cards of good manufacture were employed, but are indispensable with the ESP cards, and with various other cards which might be substituted.

3. During a test, the faces of the cards must not be visible to an assistant or to any other person, and due precautions must be taken, by lighting arrangements and otherwise, to assure the non-reception of cues by the assistant.

4. No information shall be given to any subject concerning his successes, or his scores, in the experiment, until after the termination of the service as subject.

III. *Distribution and Indications.*

1. Indicators of the five positions in which the cards are to be stacked will be cards of the same sort, from another pack. These indicators are to be plainly visible to the subject, although the stacks formed in the positions indicated are not to be visible.

2. The order of the stacks must be the same in all experiments with all subjects. For example, the order selected, from left to right, may be: *circle, cross, square, star, waves.* This order then would be observed throughout the entire experiment.

3. The subject may indicate the position in which each card is to be placed, by pointing to the indicator card, or by naming the symbol. Each subject may be allowed to choose the method (one of the two described) which he prefers, and shall then be required to adhere to this method throughout the total series of tests with him. The final report must contain notations of the method used by each subject, and certification that each subject was held to the method noted throughout the experiment.

4. The cards are to be dealt (distributed) by the assistant, in conformity with the foregoing specifications, to the stacks to which they are assigned by the subject. After laying a card on the assigned stack, (in the assigned position), the assistant shall pick up the next card from the deck, and by a single word (*e.g.,* "next") shall signal to the subject to elicit the next assignment.

IV. *Scoring and Records.*

Immediately after the completion of the distribution of the cards in any "run of the deck," the assistant shall turn the stacks over and shall record in ink the number of cards in each stack, and the number of "hits" (i.e., cards whose symbols correspond to the symbol of the stack) in each stack. These numbers are to be entered opposite the serial number for the test to which they belong, in a book ruled for the purpose of minimizing the probability of miss-entry. The entries for the several stacks must be in the same order, from left to right, as that of the stacks from the position of the assistant, and the symbol for each stack must be plainly stamped or drawn in the heading of the group of columns devoted to that stack.

Additional columns should be provided at the right of each page for the entry of total hits and misses for each run of the deck, and for such other entries as may be desirable. (See Section XI.)

No more detailed scoring of hits and misses than is above specified should be undertaken, and no computations should be made during the experimental period.

V. *Quantitative Considerations.*

1. For initial purposes twenty or twenty-five subjects may constitute a working group. On completion of work with the first group, a second group may be started, and further groups employed if and as the data accumulated indicates the need of additional numbers. This method is deemed better than that of starting a large group of subjects at one time.

2. The prescription of the minimal number of runs to be obtained from each subject offers some difficulty for two reasons. *First,* because the determination of sufficiency of runs can best be made from analysis of the actual data in the light of theoretical considerations. *Second,* on account of the unavoidable dropping out of subjects in a long experiment, the demand for sufficiency of number of runs per subject may conflict with the demand for sufficiency of number of subjects. In a critical experiment the number of runs should be high, in order that intercomparisons of fractions of the total series may be adequate, and that intercomparisons may be made with fractions of "pure chance" series. While 1000 runs per subject may be found necessary, it is possible that a smaller number may be acceptable. On the other hand, the series may need extension beyond 1000 runs. The number of runs must be the same for all subjects, as specified in Paragraph 3, which follows.

3. Each subject must be put through the same number of tests (runs) as every other subject, in so far as this is possible; and the full records of each subject must be kept, regardless of the nature of the scores. If a subject unavoidably drops out of the experiment before its completion, his scores, up to the time of his dropping out, must be preserved and reported along with the scores of the other subjects. In no case may a subject be given additional tests beyond the assigned number for the experiment. These conditions are vital for the experiment, and must be observed if statistical measures are to be applied to the data resulting.

4. Working conditions must be as uniform as possible. The length of work periods must be the same, and the number of tests per work

period must be the same, for all subjects. Since different subjects will have different rates of work, the distribution of tests through the work period should be on the familiar plan commonly known in laboratories as "headway" (*i.e.*, ten minutes headway, five minutes headway, etc.).

The distribution of work periods through the week also should be as nearly uniform as possible.

VI. *Selection of Subjects.*

As a precaution, it would be desirable to pick all subjects from a group having a relatively short range of ages and an approximation to uniformity of educational status. This condition may be met, for example, by picking from two adjacent college years, as: sophomore and junior.

Since the question of sex differences may eventually rise in connection with any experiments in this field, it will enhance the value of the data if the subjects are either all of the same sex, or else divided nearly equally between the two sexes.

As regards sophistication, there are a number of possible selective procedures, of which two are important. On the one hand, we might give preliminary tests of "ESP" to a large number of persons, and take as subjects for the experiment only those who showed a "positive" score in the preliminary tests. This method of selection would perhaps appeal to the person already convinced of the truth of the ESP hypothesis, and would not, for the sceptic, invalidate the experiment, provided full details of the method of selection were presented along with the report on the experiment. On the other hand, one might select as subjects only persons who have had no tests of any sort bearing on the question of "ESP" or other occult phenomena, and on whom therefore no preliminary ratings could be made. This method of selection would undoubtedly appeal most strongly to many experimental psychologists who are interested in all aspects of the card-sorting operation.

One or the other of these two principles of selection should be adopted in preference to any intermediate principle.

There is no objection to the use of paid subjects, provided all subjects distinctly understand that they are to be employed for the full time of the experiment regardless of their scores; and that any further employment, in similar work, after the completion of the experiment outlined, will be without pay.

VII. *Orientation of Subjects.*

Every subject should be shown a sample deck of "ESP" cards, and be informed briefly of the purpose of the experiment namely, that it is to find if, or to what extent, a subject who desires, or intends, to distribute the cards in accordance with the face-symbols, but without any sensory indication of the symbols on particular cards, can succeed in such distribution. No casual trials with a deck should be permitted; but further instruction must be given in the first actual test, the results of which must be regularly recorded.

Each subject shall be requested, as a matter of honest coöperation, to avoid, during the continuance of the experiment, handling or in any way using any deck of "ESP" cards, or of similar devices, except in the experiment itself.

VIII. *Selection of Assistants.*

The assistants must be persons who have had adequate training in experimental psychology. A graduate student who should have had at least one year of laboratory training, involving practice in methods, techniques, and precautions in a variety of experimental topics; and who has conducted individual research of laboratory character for at least a semester; would have the minimal acceptable qualification in respect to preparation. The qualification of known reliability, and carefulness in work, is especially important.

No person whose stipend or position depends in any way on the outcome of the experiments shall be employed as assistant.

IX. *Superintendence.*

The experiment should, throughout, be under the direction and control of two or more psychologists who are regarded by members of the profession generally as competent in the experimental field. One of these superintendents must be on duty during every work period, and have actual oversight of the conduct of the tests.

In view of the present situation, and the need of a definitive experiment, it is highly desirable that the experiment be set up under the superintendence of three psychologists, each from a different university.

X. *Protection of Materials and Records.*

All cards and other materials, including all records, when not in actual use, should be deposited in safe keeping, preferably in a safe, in such place and manner as the superintendents may direct. At the

end of each work period, the records for that period must be turned over to the superintendent on duty, who shall be responsible for such records.

XI. *Elaboration of Data.*

Although the details of tabulation and other elaboration of data cannot be prescribed in advance for a comprehensive experiment, there are certain points which have been indicated by past reports as of signal importance.

1. The scores for each subject should be totaled (A) for each set of five successive tests (runs of the deck); (B) for each successive set of twenty-five runs; (C) for all the tests of each work period; and (D) for the total tests of the experiment. This tabulation, in each case, should give (a) the total of hits and the total of misses for each of the five stacks separately, and (b) the totals for all five stacks together.

2. Based on the tabulated data above indicated, the *average success* of each subject, for the several groups of tests, should be computed in two ways. (A) *Average hits per test;* computed by dividing the total number of hits by the number of tests. (B) *Ratio of success* (or percentage of success); computed by dividing the total number of hits by the total number of cards.

3. Tabular or graphic presentation must be made of the distribution of scores for the whole group of subjects. This may be in terms of total hits; average hits per test, or ratio of success. This presentation must bring out clearly the critical point, which has been involved explicitly or implicitly in almost all criticisms of ESP tests, of the relation of positive scores to negative scores and to neutral scores.

The presentation should make evident, in particular, evidence as to the occurrence, in the group, of subjects falling in any or all of the three following classes: (a) Those having a success ratio (or hit average) for the total experiment, or a substantial part thereof, above the ratio which may be accepted as a typical "chance ratio" or "chance average." (b) Those having success ratios differing negligibly from the "chance ratio." (c) Those having success ratios distinctly below the "chance ratio." Changes of subjects, from time to time during the experiment, from one of these classes, to another, if such changes occur, should be made clearly evident.

4. The determination of the "chance average" and of significant deviations from this average present problems which have become con-

troversial. In a strongly skewed distribution, the relation of arithmetic mean, mode, and median is always a matter of interest; and in regard to the distribution of ESP cards there seem to be differences of opinion concerning the relation of the most probable score for a single run of the deck to the measures of distribution of a large number of scores. Although it seems hardly possible that these considerations can be decisive in the interpretation of ESP scores, nevertheless, an experiment carried to adequate length by the procedure outlined in the preceding sections may throw light on a problem which, apparently, is not to be solved by mere mathematical postulation.

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COVARIANCE METHODS IN THE COMPARISON OF EXTRA-SENSORY TASKS

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ABSTRACT: Our hypothesis is that if extra-sensory perception is present in a long series of calls, to a degree discoverable by statistical methods, it should be revealed in a comparison of various parts of the data with one another. Thus, if conditions are favorable to ESP in general, high scores on one task should be accompanied by high scores on another task; and if conditions are so unfavorable as to produce depression of scores below chance expectation, the low scores on one task should be accompanied by low scores on other tasks.

Four tasks were used, in which thoroughly shuffled cards in a cubicle at Columbia University were to be "called" by the subject: (1) Playing-cards, in which odds are 1 in 52 for direct hit and 1 in 2 for color (red or black), (2) ESP cards, in which odds are 1 in 5, (3) circles and crosses, in which odds are 1 in 2, and (4) circles and blanks, in which odds are likewise 1 in 2.

Each experimental unit is the completion of this "quadruple-task" in the order listed. Scores in one task at a given sitting are to be compared with scores in the other tasks obtained at the same sitting. The present report includes data from 1000 such experimental units of four tasks each—1000 quadruple-tasks (QTs).

The subjects in the experiment were 74 persons, most of the data coming from five major subjects, two of whom were the present writers. About half the work was done with subjects a few feet from the cards, which were on the other side of a celotex wall; the other half was done at distances of from a quarter-mile upwards.

The results indicate, in general accordance with the hypothesis, that high scores on color and suit in the playing-card task are accompanied by above-chance scores on the circle-cross and circle-blank tasks. The obtained difference between hits on the circle-cross and circle-blank tasks when color scores are high and when color scores are low is of a magnitude which should occur through the operation of chance alone about three times in a thousand.

Three of the five major subjects show the covariance of color scores and circle-cross, circle-blank scores to a markedly suggestive degree. Nevertheless, there are several phases of the distributions of the data which are difficult to interpret, and it is considered wise to continue the experiment, using new subjects as well as continuing with the old. There is suggestive evidence that awareness of one's own trends can disturb the covariance effect; for this reason those repeating the experiment are urged to avoid giving any suggestion to subjects regarding results obtained elsewhere.

INTRODUCTION

THE PRESENT study is primarily an exploratory analysis of the possibilities of a method. When the ultimate nature of a phenomenon is unknown, or its occurrence not predictable, it may be impossible to state it as a function of a specific variable, and yet the phenomenon may ren-

der itself evident in two or more ways at once, so that one manifestation of the phenomenon will accompany another. Psychologists have long recognized through the use of the coefficient of correlation the need to study the interdependence of phenomena, even when the conditions determining the phenomena remain to be discovered.

Our hypothesis is that if very slight traces of extra-sensory phenomena are present in a series of calls, they may be discovered by applying standard statistical methods to various parts of the data, to see whether such parts of the data *as should be uncorrelated on a chance basis* will show significant quantitative interrelations. We refer here to various types of measures such as are obtained by the application of the product-moment, contingency, and Chi-square methods.

Our procedure, then, was to study the efforts of subjects to call various types of concealed material at a single sitting, and to ascertain whether correct calls on one task at a given sitting are significantly related to correct calls on another task at the same sitting. If there is anything about the sitting which is favorable to ESP, the scores on the various tasks might be positively related.

Whereas most investigations in this field have been concerned with exploring the powers of unusually gifted subjects, our interest was to see what would happen when very large quantities of material were collected from ordinary persons under reasonably uniform experimental conditions. The material to be guessed by the subjects was to be definitely beyond range of all the senses, and the statistical method was to be a simple standard procedure, the applicability of which is uniformly recognized. The experiment has been in progress only about a year and a half and is as yet very far from complete. It is hoped that publication at this time will result in a variety of comments which will make possible clarification of issues and improvement of techniques and, above all, make possible the repetition of the procedure by any psychologist who has the time and interest, using groups of ordinary persons who have the time for sustained inquiries into the problems at hand.

LABORATORY AND GENERAL CONDITIONS

In a corner of GM's office at Columbia we built a 5′ x 6′ celotex cubicle, with a roof 8 feet from the floor, as shown in Figure 1. The position of the one door of the cubicle is indicated in Figure 2. This door was always shut during experimentation; the one window of the cubicle was always covered with a tight cardboard screen. Within the cubicle was a card table, upon which the cards to be guessed were placed by the experimenter, no other person being present in the office while

the cards were shuffled and arranged; the door of the office (*D* in Figure 2) was locked during this time. Subjects for the experiment were admitted to the office to make their calls, but of course were not admitted to the cubicle. GM and ET both served as experimenters.

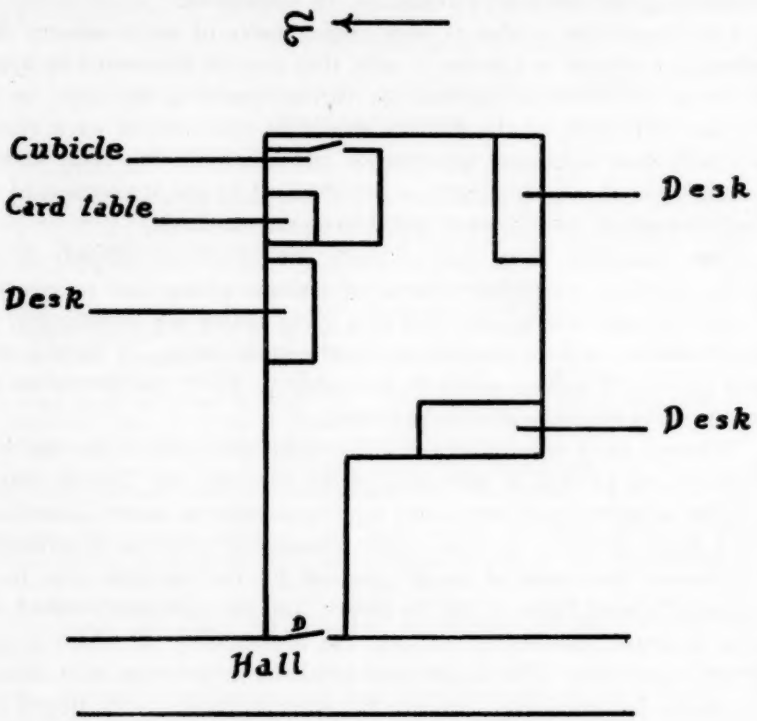

FIGURE 2. FLOOR PLAN OF OFFICE.

MATERIALS AND PROCEDURE

After considerable exploratory experimentation with the Duke University ESP cards (star, circle, cross, square, waves) we were impressed by the suggestion of a physicist that it might be wise to work with several different types of material, in order to make possible a comparison of results. It was suggested that in terms of the general psychology of perception it should be easier to distinguish between two symbols than between five, and still easier to distinguish between the presence of a symbol and the absence of that symbol. We therefore decided to set up a uniform experimental procedure in which three tasks were to be completed at a single sitting. The first of these tasks consisted of 25 of the ESP cards, to be guessed by the *down-through*

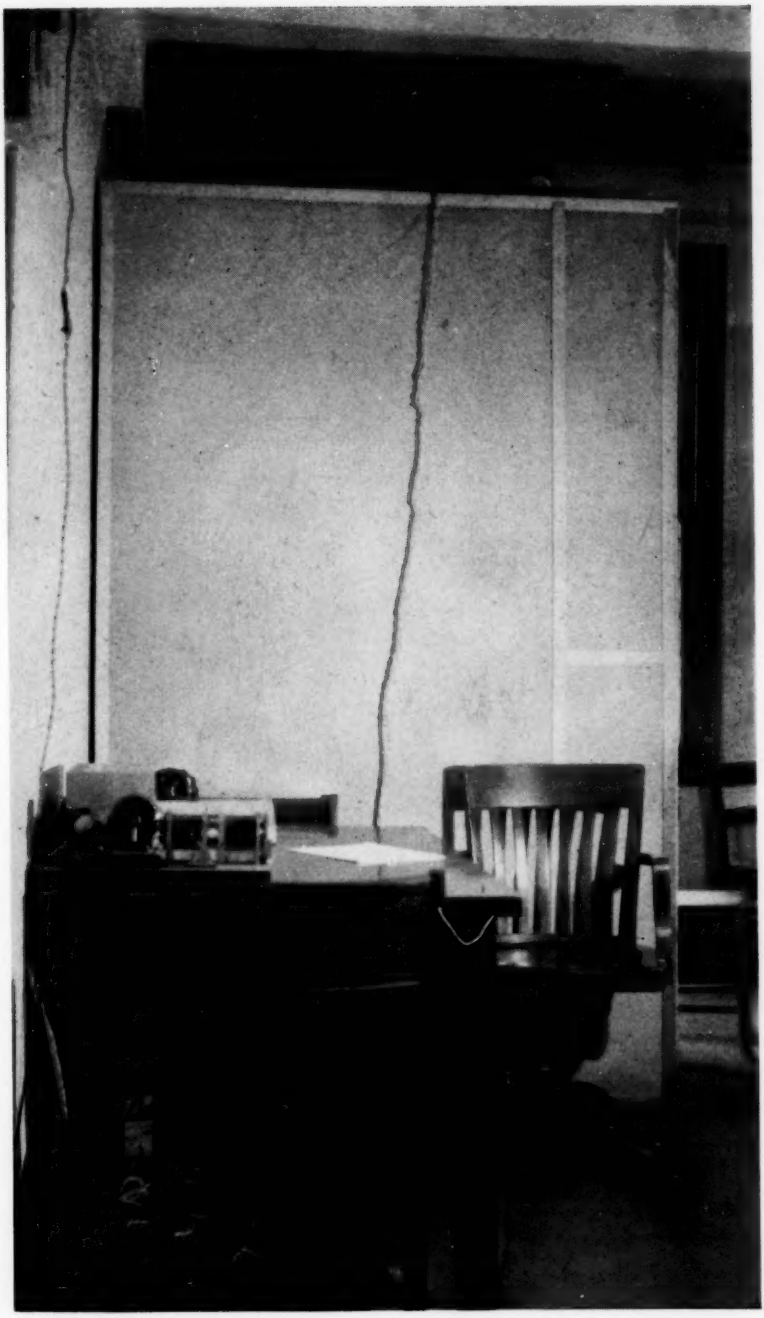

FIGURE 1. PHOTOGRAPH SHOWING OUTSIDE OF CUBICLE AS SEEN FROM ENTRANCE TO OFFICE, FACING EAST.

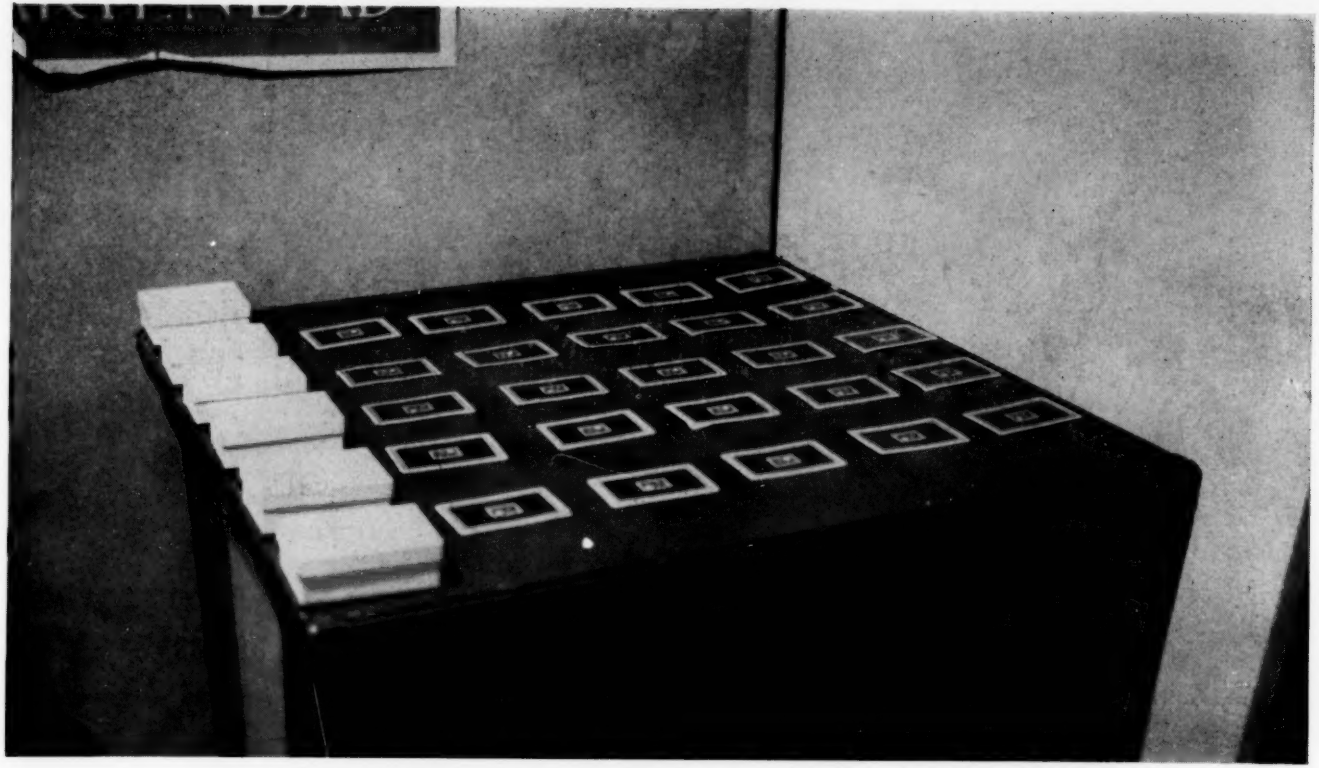

FIGURE 3. PHOTOGRAPH SHOWING DISPOSITION OF CARDS ON TABLE IN CUBICLE, FACING WEST.

method.[1] The second task was to call, down-through, a deck of 25 cards, each of which bore either a circle or a cross. In the third task, which also consisted of 25 cards, each card either bore a circle or was entirely blank.

Exploratory work with the "triple-task," as we called this procedure, made evident the necessity for a number of improvements: (1) The addition of playing-cards, in which many subjects were interested. The long odds in calling a playing-card exactly right, being 1 in 52, made the task interesting. Also, most subjects are familiar with playing-cards and *like* to guess them. (2) We felt it advisable to have two exactly comparable tasks of each type used. Consequently we made it a rule to use, for each of the three tasks, a deck of 50 cards broken into two series of 25 each.

In working with the triple-task it seemed to us desirable to prevent the subject from working by a supposed process of elimination, which would be expressed by saying, "I have called a lot of circles; I'd better not call any more of them." Although such a procedure on the part of the subject does not change the mean chance expectation, it does affect the standard deviation of the distribution of scores. This problem was partly solved in the work with the triple-task. In the case of the ESP cards, for example, 10 of each symbol were shuffled together, making a deck of 50. The deck of 50 was then dealt out, as in dealing to two persons, into two cardboard boxes, just large enough to contain the deck. Thus the distribution of symbols in a deck of 25 was not limited to 5 of each, with the result that the subject felt less compulsion to "even-up" his calls. The circle-cross and circle-blank decks were prepared in the same manner. This method of preparing decks to be called does not give an absolutely binomial distribution, but the sigma correction is slight and is easily applied.[2]

One further change in the preparation of material must be noted. Subjects found it difficult to call as many as 52 playing-cards in order, and for this reason the number to be guessed was cut to 25. This deck of 25 cards was always the top 25 cards from a shuffled deck of 52. Also, some subjects preferred to have these cards spread out on a table

[1] In the down-through (DT) method the shuffled pack of cards is to be guessed one at a time, from top card through to bottom card.

[2] At a later stage in the experiment, subsequent to collection of most of the present data, we arranged to have on hand 500 each of the ESP, circle-cross, and circle-blank cards. From each such source of 500 cards, 50 were chosen at random for a given experiment. On this basis, the maximum sigma is 1.001 times the theoretical sigma, and corrections may be disregarded. (Greenwood, J. A., and Stuart, C. E., Mathematical techniques used in ESP research, *Jour. Parapsychol.*, 1937, I, 206-225).

FIGURE 8. PHOTOGRAPH SHOWING DISPOSITION OF CARDS ON TABLE IN CUBICLE, FACING WEST.

(always within the cubicle and out of sight, as described earlier) rather than arranged for the down-through (DT) procedure; we fell into this practice until we had gotten so much data in hand as to be reluctant to change it. The scores obtained on playing-cards, as will be seen later, are nevertheless comparable with those on which the DT procedure was used. (Since under these conditions the target card cannot appear twice, there is a slight sigma correction to be made if direct hits appear markedly high or low).

The set-up just described, involving the four tasks, (1) 25 playing-cards, (2) 50 ESP cards, (3) 50 circle-cross cards, and (4) 50 circle-blank cards, was finally determined upon on December 4, 1937, and has continued to the present writing. The present report covers only the quadruple-task method, with the tasks as finally specified.

The method of administering the quadruple-task, or QT, may now be defined in detail. We shall present first the original procedure, as of December, 1937, and then discuss several minor improvements upon it which were made during the course of the present work.

The experimenter, alone in the office, with the door locked, prepared the experimental targets by the following procedure: He entered the cubicle, dovetail-shuffled[3] the entire deck of 52 playing-cards five times, and dealt out, from the top of the deck, 5 rows of 5 playing cards each, face down, on the card table, as shown in Figure 3. The order in dealing, as in calling the cards, was always left to right, top to bottom, as in reading a book. He then took the 50 ESP cards, put them through 5 dovetail shufflings, and dealt them out between two cardboard boxes, put the lids of the boxes in place, and placed the boxes in the southwest corner of the table. The procedure was the same with the 50 circle-cross cards, which were placed in the middle two cardboard boxes shown in the photograph. The experimenter then did the same with the circle-blank cards, which were placed at the southeast corner of the table. The disposition of the entire set-up is shown in Figure 3.

The experimenter then came out of the cubicle into the office, closing the cubicle door, went to the outer door of the office, and admitted the subject or subjects who wished to participate. In the experimenter's presence the subject took a record sheet[4] and indicated upon it, in order, his guesses as to the 25 playing-cards, the 50 ESP cards, the 50 circle-cross, and the 50 circle-blank cards. The experimenter then went into the cubicle, brought out the target cards, made the record of the target

[3] The pack of cards, face down, was cut by hand, and the two parts of the pack allowed to dovetail together, being then squared off by hand. The last shuffling was followed by an additional cut.

[4] A record sheet is shown in Figure 3a.

cards upon one or more of the call sheets, and scored the four tasks. The playing-card task was scored for color, suit, number and direct hit. The remaining three tasks afforded but one way of scoring—for direct hits, or correspondences, only.

Subject_____ Location of subject_____ Date_____

Time_____ P.M. A.M.—Time shuffled_____ Recorded by_____

Rate of calling_____ Time since food_____ coffee_____ smoking_____

Attitude_____ Method_____ Observer_____

Location of cards_____ Confidence scale (0-5): PC____ ESP____ O+____ O—____

General remarks_____

Series_____ Subject number_____ Target number_____

1	2	3	4	5	6	7	8	9
PC			ESP	ESP	O+	O+	O—	O—

FIGURE 3A

Beginning early in February, 1938, it was made an invariable practice to shield the call order while recording the target order. Beginning on October 5, 1938, the experimenter recorded the target order on an independent sheet before looking at call orders.

In addition to the method just described, we used a long-distance procedure, the subject making his calls wherever he happened to be, and whenever he cared to. This method, of course, required that the time of making the calls be precisely indicated on the call sheet. It was also necessary to establish a definite routine for preparing the target cards regularly each day. The times decided upon were 10 A.M. and 5 P.M. If the subject's call sheet indicated that the calls had been made while the target was being changed, the sheet was, of course, discarded.

Material of this long-distance type formed a very small portion of the total data collected up to June, 1938. But having worked mostly at short distances during the academic year 1937-8, we continued the QT method during the summer of 1938 on a long-distance basis. ET was in Denver, Colorado and in Richmond, Virginia for various parts of the summer; GM was in Berkeley, California. One other subject, RF, was in New York part of the time, travelling through various parts of the country the rest of the time. Keeping a record of the time, she called both the Columbia target and a similar target prepared in Berkeley by GM. Almost all the summer work is from these three subjects.

The California target was prepared by GM according to the method described for the New York target, and was kept in his office (which was locked except when he was there) at the University of California, a large drawer being used to spread out the cards (in the absence of a cubicle.) During the summer the New York target was prepared twice daily in the cubicle by GG, a graduate student, who regularly sent duplicate records of the New York target orders to GM at Berkeley. ET sent his calls to GM, as did RF. We resumed the QT work at Columbia in September, 1938, having at that time 597 QTs on hand; at that time we determined to complete the present series, working up the data when the number of QTs reached one thousand.

DEGREE OF ADEQUACY OF SHUFFLING

Despite the rather elaborate shuffling procedure, a test of the adequacy of the shuffling was made. The relative frequency with which each symbol appeared at various points in the deck was computed. Tables I and II give such distributions for two sets of 50 targets each, one set having been prepared by GG, the other by ET. It will be noted that the discrepancies from chance expectation are non-significant. Sec-

ond, the possibility that imperfect shuffling leads to some connection between successive targets was investigated. Targets were checked against succeeding targets, and correspondences were noted. Table III presents the results of the test, showing no significant relations between successive card orders.

An additional control is offered to test the existence of a possible linkage between different parts of the same target, e.g., whether there is any extra-chance relation between color of playing-cards at the various points in the target and the arrangement of circles and blanks in the same target. Table IV suggests no relation of this sort.

TABLE I

RESULTS OF EMPIRICAL TEST OF SHUFFLING, BASED UPON 50 TARGETS SHUFFLED BY GG, SUMMER OF 1938. THE TABLE SHOWS DEVIATIONS FROM EXPECTED FREQUENCY FOR VARIOUS SYMBOLS FOR THE FIRST FIVE AND LAST FIVE POSITIONS IN THE DECK. TWO SIGMA VALUES ARE GIVEN AT THE FOOT FOR COMPARISON.

Position in deck	TASK														
	Playing-cards	ESP deck 1					ESP deck 2					O+1	O+2	O—1	O—2
	Black	★	O	+	□	≈	★	O	+	□	≈	O	O	O	O
1	—1	—1	1	0	6	—6	2	—1	1	0	—2	—1	—9	6	4
2	—4	0	—2	1	—4	5	—2	5	0	—2	—1	—4	5	—1	2
3	—6	—2	—1	0	1	2	1	—3	0	5	—3	2	2	—7	2
4	1	—1	5	—4	—3	3	—1	—3	0	—1	5	7	4	1	0
5	—4	3	—1	—3	—1	2	—1	0	3	2	—4	—6	—2	—4	—8
21	—5	—4	—2	—2	4	4	—2	—1	7	—4	0	—1	—5	0	3
22	2	3	0	2	—1	—4	—1	—2	—2	0	5	2	3	—2	3
23	0	—2	0	2	—3	3	—3	0	0	—1	4	6	—3	1	4
24	—1	0	—2	1	—3	4	1	—2	1	—1	1	0	2	—7	5
25	—2	2	1	2	—2	—3	0	4	0	—1	—3	—5	—3	—8	—2
2σ	7	5.7										7			

TABLE II

SAME AS TABLE I, BASED UPON 50 TARGETS SHUFFLED BY ET, OCTOBER, 1938.

Position in deck	TASK														
	Playing-cards	ESP deck 1					ESP deck 2					O+1	O+2	O—1	O— 2
	Black	★	+	□	O	≈	★	+	□	O	≈	O	O	O	O
1	—4	2	0	1	—2	—1	0	4	—1	—4	1	0	0	—1	—2
2	—1	—1	—4	3	3	—1	4	—1	—1	4	—6	0	5	—1	0
3	0	—1	—1	3	—1	0	—6	1	0	1	4	3	4	—7	1
4	—4	6	0	—2	0	—4	2	—1	1	1	—3	—2	6	12	1
5	5	—3	2	5	—2	—2	—1	0	—2	1	2	—5	4	0	—1
21	—5	1	0	—1	5	—5	0	1	3	—4	0	—3	4	—6	—1
22	3	—2	0	—2	6	—2	2	2	1	—3	—2	5	—4	—4	—4
23	—2	—4	2	0	3	—1	5	—4	—1	1	—1	—1	—2	1	0
24	—1	2	—3	4	—3	0	—1	—1	1	5	—4	0	—1	5	—3
25	3	—5	4	3	—2	0	2	—3	3	—1	—1	5	3	1	4
2σ	7	5.7										7			

TABLE III

RESULTS OBTAINED COMPARING ONE HUNDRED TARGETS AGAINST SUCCEEDING TARGET

Task	Correspondences expected	Correspondences obtained	D/σD
Playing-cards (color) (25)	1250	1261	0.44
ESP (50)	1000	1026	0.92
Circle-cross (50)	2500	2553	1.50
Circle-blank (50)	2500	2492	0.25
Combined O+, O— (100)	5000	5045	0.90

TABLE IV

TABLE SHOWING AMOUNT OF LINKAGE BETWEEN PLAYING-CARDS AND THE SECOND CIRCLE-BLANK DECK. THE TABLE SHOWS THE DEGREE TO WHICH A RED (OR BLACK) PLAYING-CARD AT ANY POSITION IN THE PLAYING-CARD DECK TENDS TO BE ACCOMPANIED BY A CIRCLE (OR BLANK) IN THE CORRESPONDING POSITION OF THE CIRCLE-BLANK DECK. BASED ON 100 TARGETS.

	Playing Cards		
	Red	Black	Total
Circle-blank 2 (circle)	653	617	1270
Circle-blank 2 (blank)	618	612	1230
Total	1271	1229	2500

Chi-square 34
P 56

Evidently no significant linkage is shown here. Further data on linkage appear below, pp. 62-65.

METHOD OF SCORING[5]

The method of scoring when GM or ET was testing an individual subject during the period to the beginning of February, 1938, was to write the order of target cards in a column parallel to the record in which the subject's calls had been entered. But Kennedy[6] has shown that a graduate student opposed to ESP, recording by this method, made 9 errors in 2625 calls of the ESP cards, of which 1 increased the score, 3 decreased it, and 5 left it unaffected. It is possible that there are

[5] The question of adequacy of scoring cannot, we believe, be completely discussed to the satisfaction of all by any method in which cards are recorded by hand —one reason for the development of the machine-photographic procedure, described in another article (p. 11) in this issue of the JOURNAL. Nevertheless we wish to report as fully as we can on the problem of accuracy in recording.

[6] Kennedy, J. L., in The ESP symposium at the APA, *Jour. Parapsychol.*, 1938, II, 247-272.

considerable individual differences in the magnitude of such errors and in their direction. It will be noted later that the ESP curves, as well as the others, show no traceable effects of such a constant error, but this point is not a complete answer to Kennedy's question regarding recording errors.

During the latter part of the academic year, 1937-8, another method was used in the case of group experimentation, and this throws light upon the general question of accuracy of recording. When several subjects competively called the same target, both experimenters being present, one of them acting as subject, the other having prepared the target, the sheets were handed to the subjects to give them the satisfaction of immediate checking, while the experimenter made his own check-up, observing one of the subjects, and in the immediate presence of the others. As he turned over the cards and recorded them, he called out the order and scored one call sheet, while other subjects scored one another's papers. This, of course, was not "official" scoring; the experimenter later checked all such records, comparing them with his own target order. The number of discrepancies turned out to be approximately .001 of the total recordings. The figure just given indicates only the discrepancies found between the target order as recorded by the experimenter and the target order as recorded by subjects, making their records as the experimenter called out the order (it was noted above that shortly after the experiment got under way we began to shield the record of call orders when entering target orders).

Prior to the beginning of the Summer distance work, all data sheets were checked by the experimenters at least twice after the original scoring. Errors, as they were found, were rectified, but no record was made as to the exact number of errors found. All the data from this point on, however, were subjected to a rigid checking by various graduate students, under pressure to find as many errors as possible. 582 data sheets were re-checked on this basis, and errors as shown in Table V were found and corrected. The 418 QTs which were obtained prior to the beginning of the Summer work had been as rigidly checked, but, as stated, a record of errors found was not made.

On October 5, 1938, at which point 597 QTs had been gathered, the independent target record method was adopted, on the basis of suggestions made by Coover and Kennedy. Since the experimenter was, of course, never allowed to call a QT which he had prepared, the making of the independent target record was an integral part of the procedure in all QTs thereafter. Target orders and call orders were regularly

juxtaposed for scoring; while shielding the subject's calls, the target was recorded in columns adjacent to those in which the subject had recorded his guesses, and correspondences were then noted. Since June 6, 1938, target records have been made in duplicate. We do not know how autistic factors may work, and we wish to emphasize the correctness of Kennedy's view that it is wise to make an independent target record.

TABLE V

MAGNITUDE OF ERRORS FOUND IN CHECKING, SHOWING DIRECTION OF DISCREPANCY (582 QTS).

Task	Errors	Positive	Negative	%
Color	15	1	14	.001
Suit	4	1	3	.0003
Number	26	1	25	.002
Direct hit	2	2	0	.0001
ESP 1	18	2	16	.001
ESP 2	18	0	18	.001
Circle-cross 1	7	0	7	.0005
Circle-cross 2	4	2	2	.0003
Circle-blank 1	6	1	5	.0004
Circle-blank 2	4	1	3	.0003

Although nearly all the data were collected under our eyes, or under long-distance conditions, permitting our scoring of the call sheets before anyone else had access to them, we are entirely willing to conceive of two possible hypotheses: (1) Either or both of us is so emotional or so dissociated as to have falsified the data. (2) Someone gained access to our files and tampered with the record sheets.

Just such hypotheses as these necessitate repetition of the experiment, not once, but many times by independent groups of experimenters.

SUBJECTS

Our subjects were all those who were interested and able to give the time. No one who volunteered for the experiment was ever refused, and no one was ever discouraged from taking further tests. We did, of course, make it a practice to try subjects who were interested in the problem, but we gladly included persons of every attitude and persuasion. The present study made use of 74 subjects, the number of times each subject went through the experiment depending primarily on his or her own interest. These subjects were largely undergraduate and graduate students of both sexes, and mostly under 30 years of age.

We shall arbitrarily use the term *major subject* to include those who

did more than 60 QTs each. The largest number of QTs called by any minor subject was 52. These five persons were the present writers, a woman graduate student at Teachers College, Columbia, a woman undergraduate student at Barnard College, Columbia, and a woman who had formerly been a graduate student at Duke University, and had worked in parapsychology there.

Two groups of minor subjects may be more specifically described: (1) The Executive Secretary of the American Society for Psychical Research asked us in June, 1938, whether we would be willing to carry out some experiments with members of the society at the society rooms, located at 40 East 34th Street, New York City. We replied that we should be glad to carry out such experiments provided we could proceed in the same way in which we worked with college students, that is, under strict conditions and requiring a great deal of arduous work from each subject. The plan was approved by the trustees of the society, a small fund for apparatus and two assistants were provided, and work started in October, 1938, and continues at this writing.

The only phase of this work which is pertinent in the present connection is the QT calling which has been done by the members of the ASPR experimental group. Several members of the group came by appointment to the laboratory to do a few QTs under the regular routine conditions (sitting at desks in the laboratory or in adjoining rooms, with the cards in the cubicle, as described previously). Most of the members of the group have never seen the laboratory. Twice a week since October, experiments at the rooms of the society have included long-distance calling of the Columbia QT under the same conditions prevailing in other long-distance work and, in addition, four of the subjects have turned in regularly, at the experimental meetings, distance QTs which they had done in the time intervening between group experiments. These records received by GM and ET were scored against the previously recorded targets. We included these in the totals merely because it is uniform practice to include *all* QTs from all persons participating. The ASPR material began to come in so late that in the first thousand QTs there are only 52 from this source.

(2) On November 13, 1938, GM gave a short talk on ESP methods to about 100 students at Bennington College, Bennington, Vermont, describing the covariance method, and graphing the results from the first 817 QTs. He asked for cooperation from students in long-distance calling, which was to be under the direction of Dr. and Mrs. Dwight Chap-

man, of Bennington College. The students were to make their calls at convenient times in the day, to hand these to the Chapmans, and to have them forwarded to Columbia for checking. A number of students responded, and are continuing to call the QT at this writing.

RESULTS

The first and most obvious way to treat the data is in terms of the distribution of correspondence as compared with those expected by chance. These distributions appear in Figures 4 to 11. It is evident that the mean deviations from chance expectation on the various tasks are small and non-significant. Also, the data for the individual major subjects are fair approximations to the expected chance frequencies (all obtained scores are presented in Appendix I, arranged, by subjects, chronologically).

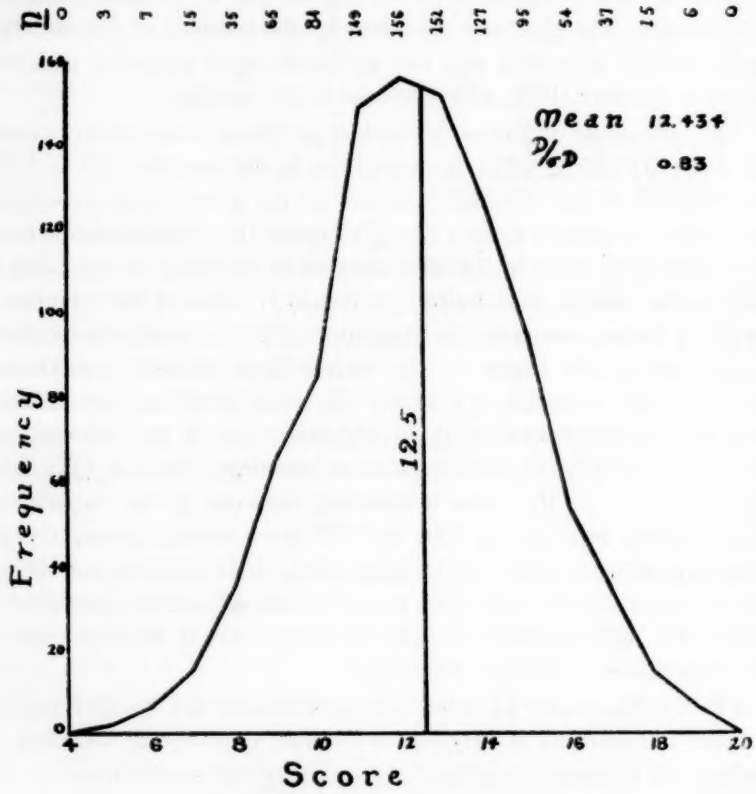

FIGURE 4. DISTRIBUTION OF COLOR SCORES.

In spite of the apparently non-significant distributions of scores, various interesting trends in the data began nevertheless to appear during the spring and summer and could not escape attention. Most striking was the tendency for high scores on the color or suit of the playing-cards to be accompanied by high scores on the circle-cross or the circle-blank task, or the sum of the two, although no association with ESP scores was apparent. Whatever it is that makes the person score high in playing-cards is for some reason correlated with a tendency to score high on circle-cross and circle-blank. A similar linkage appeared when the playing-card scores were low; a low score on color and suit in the playing-cards proved to be correlated with low scores on the circle-cross and circle-blank tasks. There was no reason, as far as we could see, why this should be the case if we were dealing with the operation of "chance," and the matter called for close study.

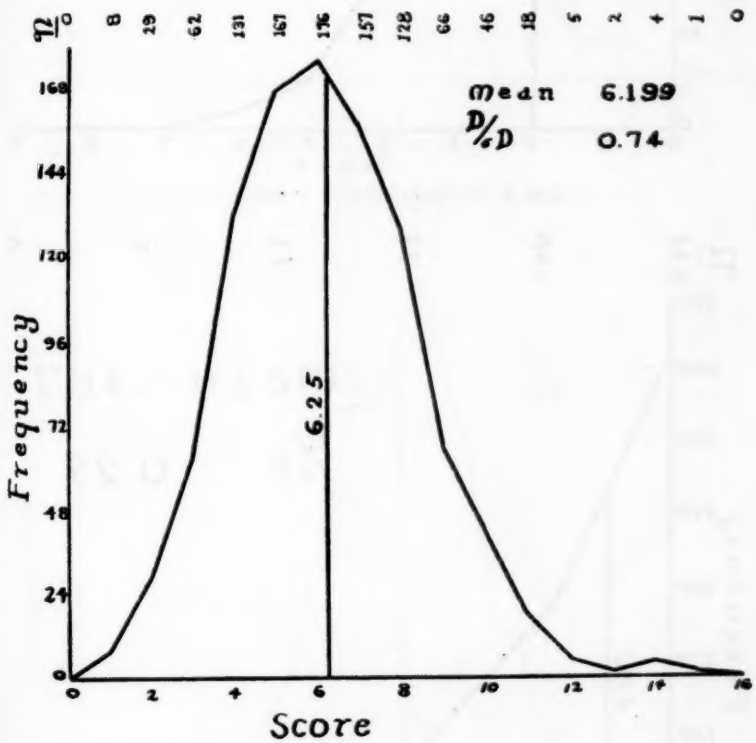

FIGURE 5. DISTRIBUTION OF SUIT SCORES.

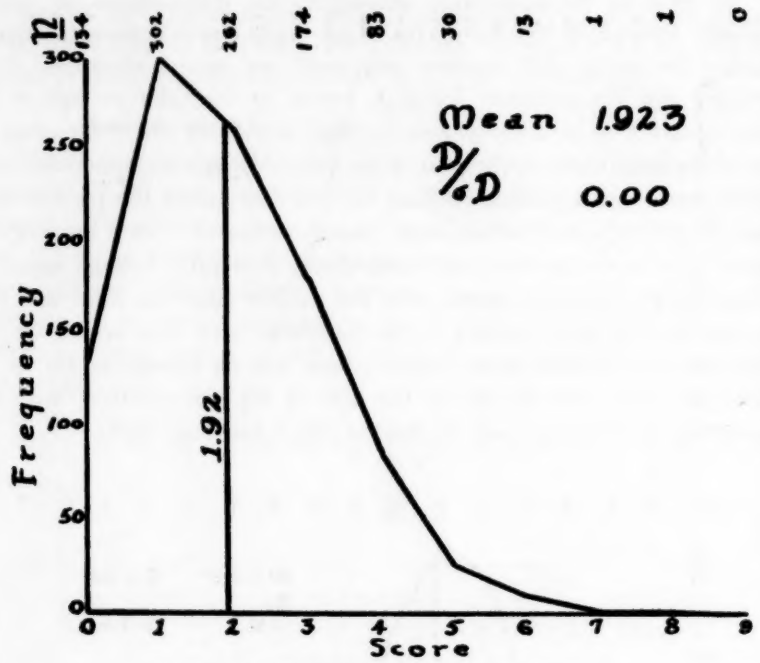

FIGURE 6. DISTRIBUTION OF NUMBER SCORES.

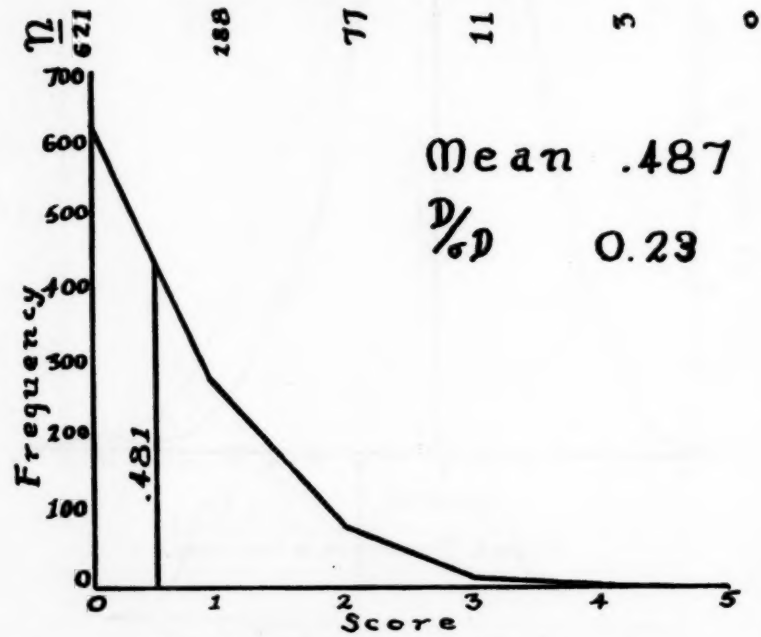

FIGURE 7. DISTRIBUTION OF "DIRECT HIT" SCORES.

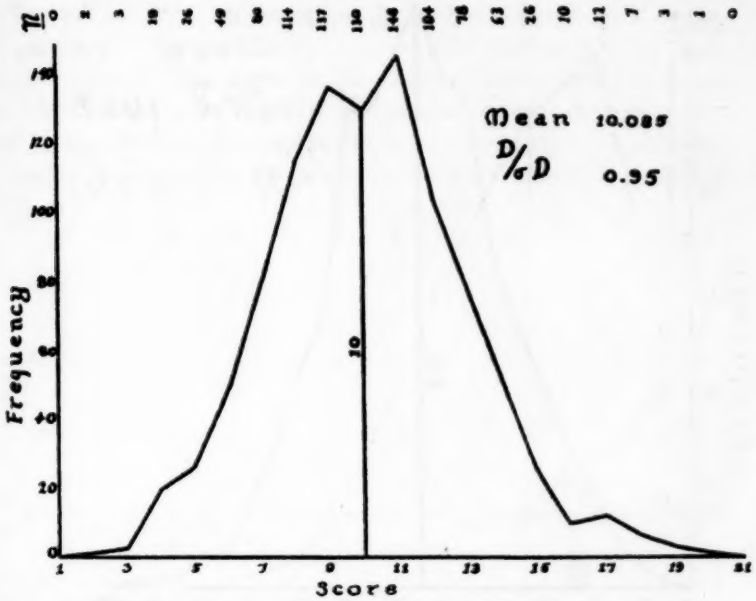

FIGURE 8. DISTRIBUTION OF ESP SCORES. (UNIT OF 50 CALLS.)

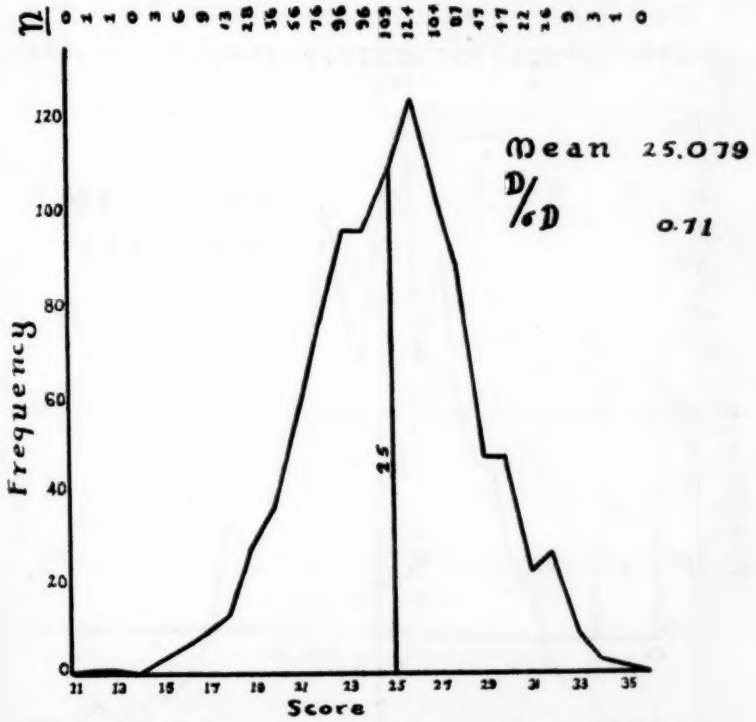

FIGURE 9. DISTRIBUTION OF 0 + SCORES. (UNIT OF 50 CALLS.)

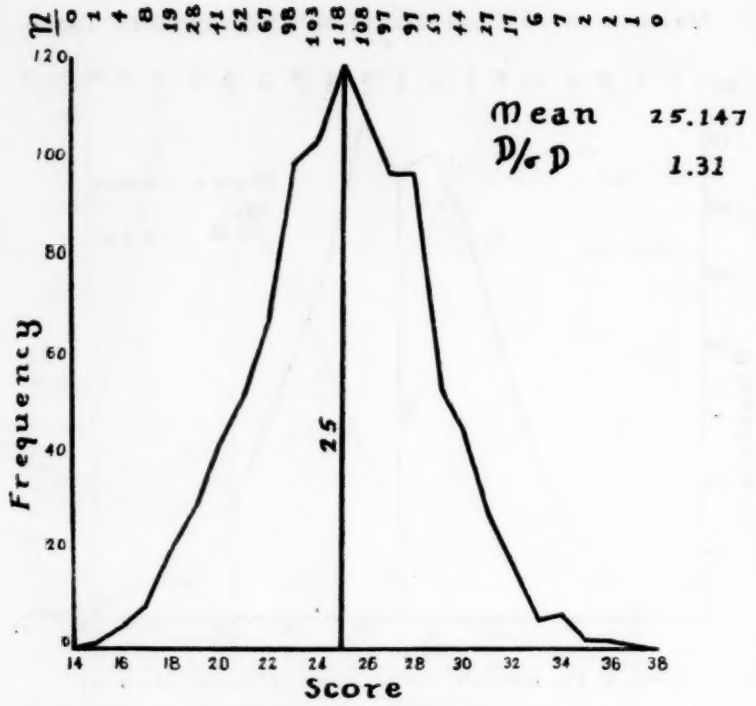

FIGURE 10. DISTRIBUTION OF 0 — SCORES. (UNIT OF 50 CALLS.)

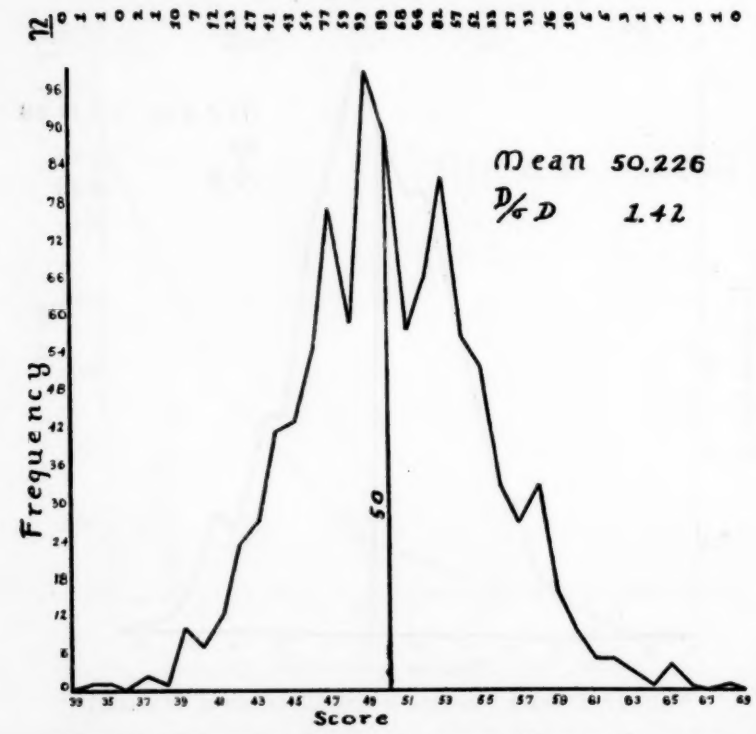

FIGURE 11. DISTRIBUTION OF 0 +, 0 — SCORES. (UNIT OF 100 CALLS

Figures 12 and 13 indicate the covariance situation as it stood at the end of the first thousand QTs. The method used in plotting the curves was as follows: The values of the *constant* score were marked on the abscissa. Then the averages of all *variable* scores obtained at the same time as the various constant scores were computed, and plotted on the ordinate. In Figure 12, for example, in which color is the constant and

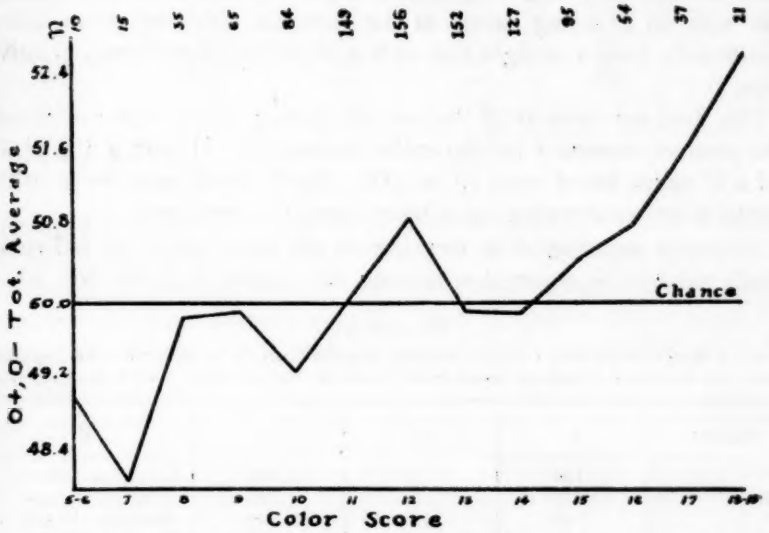

FIGURE 12. CURVE SHOWING RELATION BETWEEN COLOR AND O +, O — TOTAL SCORES, COLOR AS CRITERION.

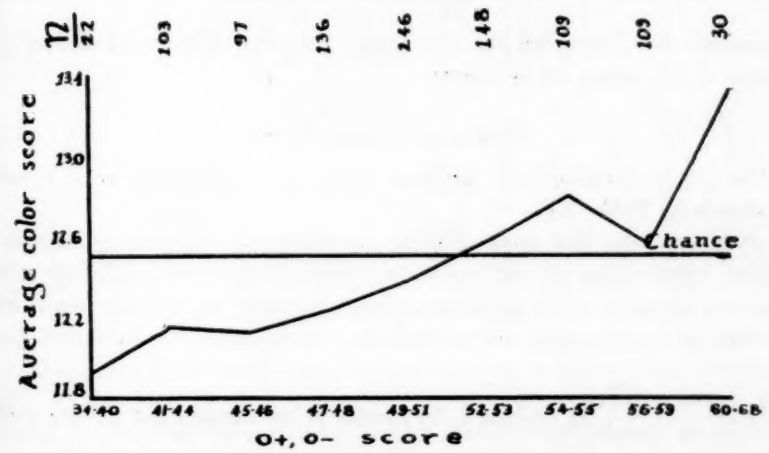

FIGURE 13. CURVE SHOWING RELATION BETWEEN COLOR AND O +, O — SCORES, O +, O — AS CRITERION.

circle-cross, circle-blank combined total the variable, the first point on the curve indicates the average of all circle-cross, circle-blank scores obtained whenever color score was 5 or 6. The number of cases upon which each point is based is given in the *N* column above the curve.

Figure 12, in which color is plotted on the abscissa, is regarded as slightly to be preferred to Figure 13 because the number of calls at each point is sufficiently large to permit a simple representation of the situation with no grouping except at the extremes. Neither curve differs significantly from a straight line with a slight, but significantly positive, slope.

This does not mean at all that we are dealing with a high correlation. The product-moment r for the entire thousand is .11 with a PE of .02 and a P value, based upon t,[7] of .003. The P based upon σr is .0002, but the more conservative value based upon t is preferred.

However meaningful or meaningless the mass trend, the individual trends need to be reported separately and appear in Table VI, which

TABLE VI

PRODUCT-MOMENT *r*S AND *t* AND P VALUES FOR FIVE MAJOR SUBJECTS. THE CORRELATIONS ARE BETWEEN SCORE ON COLOR AND SCORE ON CIRCLE-CROSS, CIRCLE-BLANK TOTAL.

Subject	n	*r*	*t*	P
RF.....	140	.27	3.336	Less than .01
GM.....	232	.14	2.144	Between .05 and .02
MP.....	69	.19	1.584	Between .10 and .20
ERT.....	62	.12	.936	Between .30 and .40
ET.....	102	.09	.904	Between .30 and .40

represents the data from the five major subjects. Figure 14 shows the scatter of the entire set of scores.

FURTHER CORRELATIONS

The intercorrelations of the four tasks were computed, with results as shown in Table VII.

It is apparent that none of these correlations is significant. In particular, circle-cross is not correlated with circle-blank. Though correlations of color with circle-cross and of color with circle-blank are positive, it is only when circle-cross and circle-blank are combined that

[7] $t = \dfrac{(n-2)^{\frac{1}{2}}r}{(1-r^2)^{\frac{1}{2}}}$ in which n is the number of comparisons and r is the coefficient of correlation.

TABLE VII

TABLE SHOWING PRODUCT-MOMENT r'S OBTAINED BETWEEN VARIOUS SCORES ($N = 1000$).

Scores	r	$r/\sigma r$	P_1*	t	P_2**
Color ESP.....	—.01	.31	.38	.316	.7 — .8
Color O+.....	.07	2.19	.02	2.212	.02— .05
Color O—.....	.05	1.56	.06	1.580	.1 — .2
Suit ESP.....	—.01	.31	.38	.316	.7 — .8
Suit O+.....	.02	.63	.26	.632	.5 — .6
Suit O—.....	.04	1.25	.11	1.264	.2 — .3
ESP O+.....	.00	.00	.50	.00	1.00
ESP O—.....	—.02	.63	.26	.632	.5 — .6
O+ O—.....	—.01	.31	.38	.316	.7 — .8

*P_1= P based upon $r/\sigma r$.
**P_2= P based upon t.

a clearly significant correlation with color is given. Whether this fact should logically suggest that the P value of .003 (in Table VIII) is an insufficient anti-chance value is partly a question for statistical judgment, but we think chiefly a point in favor of further research. If the value is an artifact, it will probably not reappear in a repetition of the experiment.

O +, O — SCORE

COLOR SCORE	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	f
19												1		1			1		1	1															1	6
18									1			1				2		2		3	1	2	1				1						1			15
17								1	1		2		2	4		5	2	1	2	3	2	4		3	1	1		1	1	1							37
16							1		1	2	1	2	1	2	5	1	4	2	5	4	5	6	3	3	2	3	1									54
15		1						2	2	1	1	3	8	5	9	14	6	7	3	8	8	5	3	3	2	1	1					2				95
14					1		2	3	1	6	2	5	7	2	12	4	16	12	5	11	10	5	7	2	2	4	2	2	1	2				1			127
13							3			1	3	4	8	8	11	10	12	10	20	7	11	14	4	3	6	5	7	3		1		1					152
12		1					1			2	3	5	6	6	6	11	7	15	14	6	8	13	10	16	7	4	6	3	2	1	2			1				156
11							1			2	2	4	5	7	10	10	14	17	11	9	11	12	12	5	2	6	6	1	2									149
10					1		2	2	1			5	6	1	4	9	6	7	5	8	6	5	3	3	6	2		1					1				84
9										1	3	3	4	6	6	3	4	10	4	5	3	3	3	1		2	1	1	1		1						65
8								2	1	1	1	2	2		2	1	3	4	2	3	4	1	1	1		1	2	1									35
7						1					1		2	1	1	1	1	1	1	1	1	1				1											15
6														1	1	1	1	1						1												7
5										1			1								1																3
F=	1	1	0	2	1	10	7	12	23	27	41	43	54	77	59	99	89	58	66	82	57	52	33	27	33	16	10	5	5	3	1	4	1	0	1	1,000

FIGURE 14. SCATTERGRAM OF COLOR AND O +, O — SCORES.

It appears to be only when the QT is done at one sitting, that is, as part of one psychological task, that the scores show the relation portrayed in Figures 12 and 13. In the same connection it is doubtful whether there is any carry-over whatever from the scores on one QT

to those on the next QT of the same subject. We hesitate to state this in quantitative terms because the time interval between successive experiments varies widely, and because the change of attitude and general psychophysiological condition may remain fairly constant for two or three experiments, even when distributed over a considerable period, or may change rapidly between experiments done one after the other in a single day.

There are several conventional ways of indicating the statistical meaning of the covariance effect for the first thousand QTs. The first is the ordinary correlation procedure, which we have already mentioned. A second is to Chi-square the data, disregarding theoretical chance expectations, comparing, for example, circle-cross, circle-blank scores when color is high, and circle-cross, circle-blank scores when color is low, to see if the two sets of circle-cross, circle-blank scores derive from the same parent population. A negative finding here would indicate the operation of extra-chance factors. A third procedure is to throw out scores at the middle of the distributions and simply compare high against low (the variable scores when the constant score is low and the variable scores when the constant score is high). As Table VIII shows, each of these three methods gives a value somewhat higher than ordinarily regarded as necessary for significance. We should be the last, however, to insist on any such conception as that of absolute reliability. In this, as in all other experiments in borderline fields, the problem seems to us to be one of accumulating and organizing data in such a way as to permit the reader to form his own hypotheses.

TABLE VIII

TABLE SHOWING P VALUES OBTAINED BY THREE DIFFERENT METHODS OF ANALYSING COVARIANCE BETWEEN COLOR SCORE AND SCORE ON CIRCLE-CROSS, CIRCLE-BLANK TOTAL. BASED ON 1000 QTS.

Analysis	P
Chi-square[1]	.006
High-low[2]	.0004
Correlation	.003

[1]Color scores when circle-cross, circle-blank total score is less than 48 against color score when circle-cross, circle-blank total score is more than 52.
[2]Significance of difference between color mean when circle-cross, circle-blank total score is above 52 and when circle-cross, circle-blank total score is below 48.

The data were subjected to a complete Chi-square analysis, in order to determine the degree to which each of the various circle-cross and circle-blank tasks contributed to the mass trend shown in Figures 12 and 13. The method was as follows: In the case of each of the criteria

(the circle-cross and circle-blank decks, separately and in various combinations) the color scores obtained when the criterion was high (top sixth of the distribution of scores) and when the criterion was low (bottom sixth of the distribution) were compared by the Chi-square method, using the simple fourfold contingency table. The obtained results are presented in Table IX.

The table shows that each of the four decks contributes positively to the mass covariance effect, but that the first deck of circles and crosses contributes almost not at all, while the second circle-cross deck contributes to a considerable degree ($P = .0004$). Also, it is seen that although neither of the two decks of circles and blanks contributes significantly to the covariance effect when considered as a unit of 25 cards, these same decks, considered as a single unit of 50 cards, contribute to the covariance effect to a suggestive degree ($P = .006$).

Also, when the total circle-cross and circle-blank tasks are considered to be two units of 50 cards each, the covariance gives a P value of .0001, the most significant value in the table, whereas when the last four tasks are considered as a single unit of 100 cards the P value is .01.

An important phase of the covariance figure as a whole is seen in the material indicating the trend of the low part of the curve. To be low on the first task gives a suggestion as to low scores on the last two tasks. This type of phenomenon, if genuine, necessitates, as Coover[8] points out, a hypothesis about *negative extra-sensory perception.* One must in a sense know where a target is in order to miss it. This was intensively studied by Rhine[9] in a long series of trials. From the present data it seems likely that a slight trace of this negative tendency may be one of the results to be expected in routine clairvoyance tests.

With due regard to the probable errors, it appears that the covariance is present in the work of all major subjects, that is, those who have done 60 or more QTs, but it is not clearly present, as far as can be seen, in the minor subjects. At the present writing it is uncertain whether the minor subjects will in time show the covariance in accordance with the trends shown in the curves of Figures 12 and 13. It is also possible, of course, that the subjects who drop out are among those who, for some reason or another, lack the particular psychological dispositions upon which the covariance ultimately depends. It would be premature to define the cause of this difference between major and minor subjects. When once the covariance appears, however, it seems to continue rather steadily, as will be seen later.

[8] Coover, J. E., This issue of the JOURNAL, p. 17 ff.
[9] Rhine, J. B., *Extra-Sensory Perception,* 1934, p. 62 ff.

TABLE IX

RESULTS OBTAINED BY CHI-SQUARE ANALYSIS, COMPARING COLOR SCORES OBTAINED WITH UPPER SIXTH OF DISTRIBUTION OF CIRCLE-CROSS, CIRCLE-BLANK TOTAL SCORES AGAINST COLOR SCORES OBTAINED WITH LOWER SIXTH OF DISTRIBUTION OF CIRCLE-CROSS, CIRCLE-BLANK TOTAL SCORES.

	Task compared with color[1]	Mean 1[2]	Mean 2[3]	χ^2	P
1	First circle-cross (a)	12.29	12.32	.01	.92
2	Second circle-cross (b)	11.91	12.79	12.82	.0004
3	First circle-blank (c)	12.22	12.54	.17	.32
4	Second circle-blank (d)	12.22	12.51	.14	.29
5	a, b, c, and d	12.16	12.53	9.50	.002
6	b, c, and d	12.12	12.61	12.30	.0005
7	a and b	12.09	12.54	6.75	.009
8	c and d	12.22	12.53	3.03	.08
9	Total circle-cross (e)	12.05	12.79	6.65	.009
10	Total circle-blank (f)	12.15	12.92	7.36	.006
11	e and f	12.10	12.85	14.03	.0001
12	Total circle-cross, circle-blank	12.14	12.81	6.39	.01

[1]In rows 1 to 8 the tasks are considered as separate units of 25 cards each. In rows 9 to 11 the circle-cross and circle-blank tasks are considered as separate units of 50 cards each. In row 12 the circle-cross and circle-blank tasks are considered as a single task of 100 cards.

[2]Color mean obtained by averaging all color scores obtained with lower sixth of circle-cross, circle-blank total distribution.

[3]Color mean obtained by averaging all color scores obtained with upper sixth of circle-cross, circle-blank total distribution.

Some of the data *since* the first thousand, which cannot yet be adequately reported, suggest that too much knowledge of one's own results may destroy the covariance effect, at least in certain subjects; this is a separate question with respect to which we are not as yet ready to report.

When there is reason to suspect that a subject is manifesting, during successive QTs, a more or less consistent tendency to covariance between color scores and circle-cross, circle-blank scores, we need a unit by means of which to measure the trend and ascertain its significance. Such a unit is provided by the product, in sigma units, of the deviation from chance on one task and the deviation from chance on another task, with due regard to algebraic sign. The theoretical sigma of the color score, where the number of calls is 25 and the chance expectation is 12.5, is 2.5 ($\sqrt{npq}$), and since the sigma of the circle-cross, circle-blank score, where the number of calls is 100 and the chance expectation is 50, is 5, a subject who made a score of 15 on color, and 55 on circle-cross, circle-blank would be one sigma above chance on each, and the resulting product would be 1 x 1, or one positive unit.

Similarly a subject with a score of 15 on color and 60 on circle-cross, circle-blank would be 2 units above chance; a subject making 15 on color and 45 on circle-cross, circle-blank would be one unit below chance,

in terms of this product of a positive and a negative quantity. When color and circle-cross circle-blank scores are both less or both greater than chance expectation we have a positive value in the covariance score; when one is above chance, the other below, we have a negative covariance score. This procedure is simply a phase of the ordinary computation of the product-moment correlation. But it has the value of revealing the general trend of a subject over a large number of QTs. All that need be done is to ascertain the positive covariance, and the negative covariance, subtract the latter from the former, and ascertain the significance of this residual positive covariance.

In Figure 15 are presented cumulative covariance curves for the five major subjects, plotted as just described. Each point on the abscissa is the number of QTs completed at that point; the algebraic sum of all covariance shown at a given time is plotted on the ordinate. On this basis a steadily mounting curve means consistency in the manifestation of the covariance effect; an irregular curve, cutting the base line frequently or remaining near it would mean the absence of a clear covariance effect. The curves shown in Figure 15 clearly indicate the degree to which the covariance appears for a given major subject in all work up to any given point (since the points on the curve are averages of 5 values, terminal values, if used to compute *r*, should be multiplied by 5.) The significance of each of these curves can be determined from the values presented in Table VI. The values in the P column represent

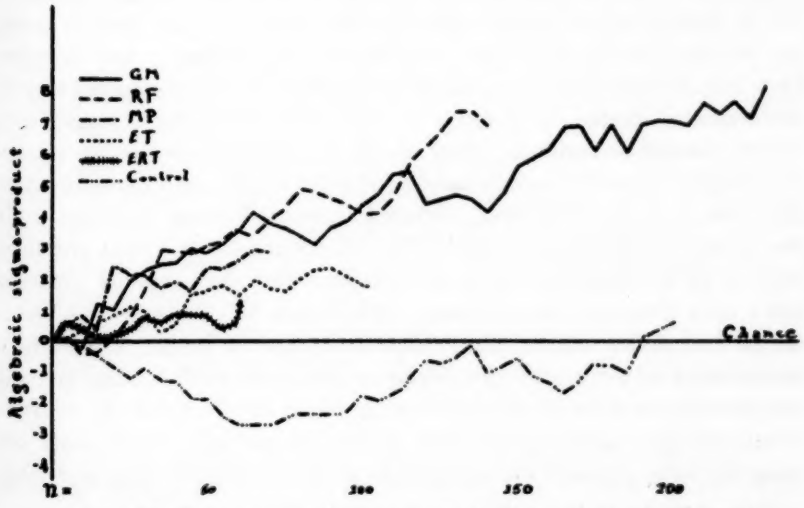

FIGURE 15. CUMULATIVE COVARIANCE CURVES FOR FIVE MAJOR SUBJECTS.

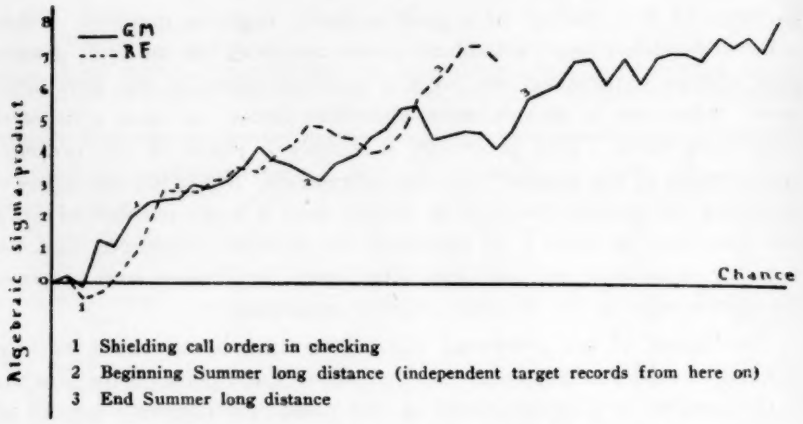

FIGURE 16. CUMULATIVE COVARIANCE CURVES FOR TWO MAJOR SUBJECTS.

the probabilities that the individual *r*s for each subject are due to chance; since each *r* is determined by the calculations involved in plotting the curves in Figure 15, the P values in Table VI give the probabilities that the final residuals of positive covariance, represented by the last points on each of the curves, are chance effects. The *control* curve was obtained using scores of targets checked against *succeeding targets.*

THE PROBLEM OF LINKAGE

It has been pointed out[10] that in order to determine whether the covariance effect is genuine it is necessary to investigate the possibilities of *linkage* in the targets called by the subjects. Any linkage existing between two parts of the same target, any tendency, for example, for a red playing-card in the playing-card deck to be accompanied to an extra-chance degree by a circle in the circle-blank deck would contribute toward covariance. That is, if the subject consciously or unconsciously responds to such a linkage in the targets, and tends to adopt that linkage in his own calls, obtained scores will show covariance. If the linkage in the target is such that red playing-cards in the one deck tend to be accompanied by circles in the other deck, and if the subject's calls show the same linkage, *both* scores, i.e., the subject's scores on the two decks will be either abnormally low or high. Therefore it is necessary to investigate the targets to see if any such linkage actually was present, in spite of the fact that in much of the work the subjects never did have access to records of the targets and hence could not normally have obtained information as to the presence of any such link-

[10] See *Letters and Notes*, this issue, page 118.

age. The 140 targets called by subject RF (the most striking case of covariance) were subjected to such a test. The method was as follows:

1. *Linkage between circle-cross and circle-blank decks.* Each of the 140 targets called by RF contains a deck of 50 circle-cross cards and a deck of 50 circle-blank cards. It was desired to determine the linkage, or association, between the two decks, to determine, in other words, whether the presence of a circle (or a cross) at any point in the circle-cross deck had any predictive value as to the presence of a circle (or a blank) in the same position in the circle-blank deck. The presence of such association may readily be tested through the use of the four-fold contingency table:

		Circle-cross deck: Circle	Circle-cross deck: Cross
Circle-blank deck	Circle		
	Blank		

We have two decks of cards, then, each of which has attributes A (circle) and B (cross or blank). If there is no relation between the two attributes, we expect to find the same proportion of A's among the B's as among the non-B's. The fourfold contingency table, as shown above, provides the required information; through the use of such a table, Chi-square is obtained, which may be translated into terms of probability (P) that such a result is to be expected through the operation of chance alone.

The 140 targets called by RF were broken down with respect to different shufflers (experimenters), and the results obtained are presented in Table X. It is seen that the P values obtained are well within the limits of chance, so that no known linkage obtains between these two parts of the target.

As will be seen later, it is necessary to consider the *direction* of any linkage which might be present. Thus we may arbitrarily define as *positive* the tendency for a circle in the circle-cross deck to be accompanied by a circle in the circle-blank deck, as *negative* the tendency for a circle in the circle-cross deck to be accompanied by a blank in the circle-blank deck.

TABLE X

P VALUES OBTAINED IN MEASURING AMOUNT OF ASSOCIATION BETWEEN CIRCLE-CROSS AND CIRCLE-BLANK DECKS IN 140 TARGETS CALLED BY RF.

Shuffler	N	P	Nature of linkage
GG.....	58	.18	Positive
GM.....	19	.48	Negative
ET*.....	63	.58	Negative
Total.....	140	.82	Positive

*On eight occasions ET's schedule made it physically impossible to be on hand at a stated time (10 AM or 5 PM) for the shuffling of a long-distance target. On these occasions other students, trained in the procedure, did the shuffling. We regret that these exceptions occurred in the routine. The eight targets differ in no evident way from the remaining 55 targets prepared by ET, nor were the hit or covariance scores out of the ordinary range. Since we are making it a rule to exclude no records, these are pooled with the ET series.

It is of interest to note that the linkage which is present is reliable for none of the series, and also is inconsistent, the P value for the entire 140 targets indicating almost no residual association, the various positive and negative trends cancelling each other when combined.

2. *Linkage between playing-cards and circle-cross and circle-blank decks.* The test for this linkage was the same as previously described; in this case the degree of association between the playing-card deck and each of the circle-cross and circle-blank decks was determined. The same targets were used—the 140 called by RF. The obtained values are presented in Table XI.

TABLE XI

P VALUES OBTAINED IN MEASURING AMOUNT OF ASSOCIATION BETWEEN PLAYING-CARDS AND CIRCLE-CROSS, CIRCLE-BLANK DECKS IN 140 TARGETS CALLED BY RF.

Shuffler	N	O + 1	O + 2	O — 1	O — 2
GG.....	58	.17 (+)*	.59 (—)	.60 (+)	.40 (+)
GM.....	19	.29 (+)	.03 (—)	1.00 (0)	.84 (—)
ET.....	63	.43 (—)	.25 (—)	.67 (+)	.79 (+)
Total.....	140	.45 (+)	.06 (—)	.53 (+)	.52 (+)

*The algebraic signs in parentheses indicate the nature of the association. (+) indicates a tendency for a red playing-card to be accompanied by a circle, (—) by a blank.

Here again the P's are not of great significance, although in the case of the second circle-cross deck the nature of the association is consistently negative. More will be said of this later.

It was thought desirable to determine the same linkages for the *calls* of RF. Using the same methods, then, these computations were made, the results being presented in Tables XII and XIII.

Comparing Tables XI and XIII it is seen that although the linkages for the calls are generally more pronounced than the linkages for the cards, the total values do not approach the point of complete reliability.

TABLE XII

P VALUES OBTAINED IN MEASURING AMOUNT OF ASSOCIATION BETWEEN CIRCLE-CROSS AND CIRCLE-BLANK *calls* OF RF.

Shuffler	N	P	Nature of linkage
GG	58	.04	Positive
GM	19	.26	Negative
ET	63	.04	Positive
Total	140	.02	Positive

TABLE XIII

P VALUES OBTAINED IN MEASURING AMOUNT OF ASSOCIATION BETWEEN PLAYING-CARD AND CIRCLE-CROSS, CIRCLE-BLANK *calls* OF RF.

Shuffler	N	O + 1	O + 2	O — 1	O — 2
GG	58	.27 (+)	.73 (—)	.19 (—)	.44 (+)
GM	19	.07 (+)	.58 (+)	.14 (—)	.78 (+)
ET	63	.89 (+)	.02 (+)	.11 (+)	.26 (+)
Total	140	.13 (+)	.17 (+)	.29 (—)	.17 (+)

An interesting fact which emerges from the tables is that while the greatest association between playing-cards and the other decks is between playing-cards and the second circle-cross deck (Table XI), and is *negative,* the linkage in the subject's calls on the same two decks is *positive.* Such a finding tends to make the covariance effect the more striking, since it is obtained in the face of a linkage in the direction which would produce negative covariance, whereas the actual covariance of the subject's scores was positive. And in general, consultation of the various tables indicates that the covariance effect, as demonstrated in the present experiment, is probably not attributable to linkages of the sort studied, either in the targets or in the subject's calls.

DISCUSSION OF RESULTS

Since we are not dealing with an extra-sensory function spread throughout the single QT as a whole, it is incumbent upon us to venture a guess as to why task 1 is related to tasks 3 and 4, and not to task 2 (ESP cards). One possible reason suggests itself; the odds in calling ESP cards correctly are 1 in 5, while in calling for color in the playing-cards, and in calling the circle-cross and circle-blank tasks, the odds against correct calls are 1 in 2. Anyone who calls the QT a number of times will see what is meant by saying that the attitude depends a great deal upon the odds. If one mixes 5 circles with 45 crosses, for example, and asks the subject to locate the circles, using only five guesses, the

situation is markedly changed. It is possible that the attitude in calling the circle-cross and circle-blank tasks, and in calling the colors, in the playing-card tasks, is a consistent "fifty-fifty" attitude; most subjects, when they see their scores on the playing-cards, are likely to remark, "I got so and so many colors right," compared always against the 12.5 chance expectation. This means that the *red versus black,* or fifty-fifty situation is real to them, and the *attitude,* in terms of fifty-fifty odds, can well carry through tasks 1, 3, and 4.

Now the scores on suit, where the odds are 1 in 4, are consistently, for all the data, just about one-half of the scores on color. The covariance of suits against circle-cross, circle-blank is the same as for color, the only difference being that the regression lines for suit are slightly less reliable than for color, because of having only half as much material for comparison (theoretical expection being for half as many suit correspondences as for color correspondences).

The fact that the suit score is half the color score, however, appears to mean that suit, as a designation *independent of color,* is not contributing to the covariance at all. A comparison of suit scores with circle-cross, circle-blank scores gives the same result as a comparison of "color minus suit" scores, that is, the scores on color hits which are not suit hits. This means that suit scores correlate with circle-cross, circle-blank scores only because in order to call the suit correctly one *must* call color correctly. Now the suit odds, being 1 in 4, are comparable to the ESP card odds of 1 in 5, and as such have little in common with the attitude involved when odds are even. When the "set" is for the fifty-fifty odds, nothing beyond chance occurs in tasks where the odds are other than fifty-fifty.

In harmony with this interpretation is the fact that "number" scores on the playing-card task, that is, correspondence in terms of ace, 2, 3, etc., show no covariance with the circle-cross and circle-blank tasks. The odds here are 1 in 13. Also, direct hits on the playing-card task, in which odds are 1 in 52, show no covariance with circle-cross and circle-blank scores. But of course this negative evidence is of slight weight.

It may well be asked whether a slight ESP function, if present, is spread evenly through the single QT or whether it comes by fits and starts, as if a faucet or light were turned on and off. This question can be answered with fair certainty by analyzing long runs of right or wrong guesses. If there is any tendency for a positive ESP function to intervene prominently at a given point and to remain present for a few seconds, it should produce a string of correct calls; similarly a negative factor of prominence should cause a string of incorrect calls.

The 25 playing-cards, the 25 cards in each box of circle-cross cards and in each box of circle-blank cards were broken into arbitrary units of 5 cards, and the total number of units of 5 correct and 5 incorrect was tabulated for the thousand QTs. The chances of getting a unit of 5 all correct, where odds are 1 in 2 per call, is of course approximately 1 in 32, so that for the entire thousand QTs the number of units of 5 right should be 125,000 divided by 32. The empirical data agree closely with this figure. So also with the boxes of all-wrong guesses. There is, so far as we can see, *no bunching* of hits, as tested by this criterion. It seems to us that whatever is present which is favorable or unfavorable carries rather evenly through a single QT.

A TENTATIVE PSYCHOPHYSICAL HYPOTHESIS

On general psychophysical grounds the way in which such a function, whenever present, would be conceived to work would be as follows: The tendency to make a given call, let us say a circle when one confronts the circle-cross task, is variable, depending upon many factors, in a word, the whole *set* or disposition of the moment. Sometimes the tendency to call a circle is very strong, the tendency to call a cross much weaker. In other instances the tendencies to circle and cross may be almost evenly balanced. A very faint ESP function would be of little or no consequence in the former instance, but of considerable consequence in the latter instance. Thus when the bias for circle is strong, a weak ESP function indicating cross could accomplish nothing in this call, even if present in maximal degree, and of course an ESP function indicating circle would also make no objective difference, when the card actually was a circle, i.e., if an ESP tendency is in harmony with the *prevailing disposition* it can accomplish nothing objectively, and if it is competing against a much more powerful force it can also accomplish nothing. It is *therefore* only when the tendency to circle and cross are about evenly balanced that we can get any expression of an ESP function. If, however, the factors affecting normal thresholds are normally distributed, we should expect the raw score in a given task to depend first upon the accidental correspondence of associative habits and target orders, second upon the amount of ESP present through the QT in question, and third upon the exact way in which the ESP function is able to overcome contrary associative habits so as to "trip the triggers" of the various calls in which the subject is almost evenly poised between two tendencies.

Since the ESP function could on this basis manifest itself only

very infrequently in the course of a series of calls, it would not be likely to reveal itself in both the even-numbered and the odd-numbered elements of a short series, so as to give rise to an appreciable odd-even reliability. We have attempted several mathematical analyses to predict the odd-even reliability of tasks of 25 and 50 calls when ESP contributes only slightly to the variance. The resulting values are so low that it is doubtful whether they can be interpreted; they tend to be only slightly greater than the theoretical PEs of very low correlations with this material, and therefore incapable of giving clear statistical meaning. We doubt the value at present of a detailed mathematical analysis of the conventional odd-even reliability of scores when the covariance effect is of the present low magnitude. But we shall be glad for suggestions as to meaningful ways of going beyond the sheer covariance effect as such.

It is perhaps of some importance here to consider the problem of maintaining the subject's morale throughout the long series of trials required. The present writers have found it easiest to maintain morale in the case of group experimentation, in which a small and congenial group of subjects calls the QT in a more or less competitive manner. Of importance is the immediate checking of call sheets, which enables the subjects to know how they are scoring, and gives them a definite sense of contact with the experiment, in which case interest is generally maintained at such a level that subjects wish to go on with the experiment. If a subject scores high, he generally wants to see if he can do it again, and if he scores low he immediately wants a chance to do better.

The subjects must not be informed of the covariance effect; the experimenter may, by manifesting interest in other aspects of the calling, focus the attention of the subjects upon some other phase of the data than the covariance.

In the present research, including some material beyond the first thousand QTs, it appears that in some of the work done under conditions other than just described the covariance is not present to the degree indicated in the group experimentation. It is possible that the type of morale generated by group work is considerably more favorable for the manifestation of the covariance effect than are conditions in which subjects are isolated, but we are not yet ready to state this in quantitative terms. We do wish to emphasize, however, that the covariance effect is apparently, to some extent, a function of the morale of the subject. A specific testing of this hypothesis is at present in progress.

CONCLUSIONS

Two defensible interpretations of our data appear conceivable, either (1) the covariance effect may be a genuine result with certain subjects working under certain conditions, or (2) it may be an extremely unusual and at present obscure statistical artifact. Which one, if either, is the correct interpretation can only be settled by further investigation.

At present our chief interests are (1) in discovering whether we can repeat the covariance results with new subjects and new methods, (2) in testing covariance results with the machine described on p. 11, in this issue, and (3) in getting other laboratories to repeat the experiment, either by calling their own targets or by calling machine-set material at Columbia, keeping their own records and, if convenient, sending us daily duplicates. As a final word of warning, those repeating the experiment are urged, for reasons already mentioned, to keep subjects in ignorance of the covariance effect as reported in the present paper.

APPENDIX

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP* (1)	*ESP* (2)	O + (1)	O + (2)	O − (1)	O − (2)
CA	16	10	5	1	8	2	11	13	14	12
	13	4	1	0	8	5	11	14	14	10
	14	7	2	1	2	6	12	12	12	14
EA	12	5	4	0	6	5	13	14	12	9
BA	11	5	0	0	2	2	11	15	10	11
	17	9	4	2	2	5	11	16	13	12
	13	9	1	0	2	4	11	13	14	9
	9	5	1	0	5	3	13	9	11	17
	14	5	0	0	3	8	13	14	12	12
	8	5	0	0	5	5	12	11	11	11
	14	5	6	1	5	5	19	14	14	11
	11	4	3	1	7	6	13	15	9	9
	13	9	2	0	10	7	14	12	8	16
	13	10	2	0	5	9	13	17	11	15
	11	2	2	0	4	7	15	10	13	12
	10	6	4	2	5	4	9	8	18	12
	14	5	1	0	2	2	13	16	16	12
	10	5	3	0	5	6	13	16	14	14
Br	15	8	1	1	7	8	12	15	12	12
AB	12	7	3	1	8	7	17	12	13	7
	11	6	1	0	6	10	13	13	13	13
	13	8	1	1	5	5	10	11	12	10
	12	8	0	0	7	5	12	10	10	6
	8	2	3	0	3	4	13	14	16	16
	10	6	1	0	4	5	12	16	17	14
RB	6	3	4	2	6	5	13	12	10	13
FB	10	5	2	0	10	4	15	17	8	13
	14	8	1	1	3	5	14	12	10	11
	10	8	2	2	4	4	13	9	16	9
	16	10	6	3	5	6	16	14	10	12

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP* (1)	*ESP* (2)	O + (1)	O + (2)	O − (1)	O − (2)
FB	12	5	1	0	5	4	14	10	10	14
DC	14	12	0	0	7	6	16	10	16	10
	17	10	1	0	5	7	10	11	15	13
	12	7	1	1	9	5	15	12	12	13
	16	5	0	0	4	3	13	13	14	18
	13	7	2	0	5	9	14	12	12	11
HC	14	9	1	1	10	7	14	13	11	14
	15	7	2	1	6	3	10	8	18	13
	11	4	3	0	5	6	15	10	14	11
	19	8	3	0	9	5	17	5	13	12
	16	8	0	0	6	10	14	13	11	9
	15	10	4	2	0	5	11	11	9	10
	15	5	1	0	4	9	11	10	11	16
KC	15	10	1	1	6	4	10	14	11	14
	12	6	0	0	6	6	14	17	14	15
	15	8	2	0	4	8	15	12	18	12
	12	6	1	0	8	6	13	13	10	13
	13	6	0	0	5	5	12	14	11	13
MC	14	8	4	1	5	6	10	9	14	15
	8	3	2	1	4	4	15	16	14	14
	12	5	2	0	3	5	15	9	14	18
	14	8	2	0	4	8	13	15	9	10
	11	3	3	0	4	4	14	13	12	15
	12	7	0	0	4	3	10	15	14	15
	10	6	2	1	9	6	12	13	16	10
	16	7	1	1	4	7	10	11	11	15
WC	15	8	0	0	5	2	11	8	12	11
Da	14	7	1	1	8	4	10	11	10	13
	12	5	1	1	5	3	12	11	11	15
	12	7	4	1	5	7	16	12	12	16

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	O + (1)	O + (2)	O — (1)	O — (2)
Da	14	8	3	1	5	4	14	12	11	13
ND	13	6	2	0	8	5	12	18	11	12
	10	5	3	0	9	10	15	14	8	16
	11	8	1	1	8	5	13	14	12	11
	9	5	0	0	3	4	8	12	13	15
	10	6	1	0	8	6	15	11	9	16
BE	13	4	4	0	5	4	11	9	11	13
	13	6	3	0	7	5	13	10	12	13
	16	10	0	0	5	6	12	11	11	8
RF	14	2	1	0	2	5	15	12	14	8
	15	8	1	0	7	3	16	12	9	11
	11	7	0	0	5	4	8	19	11	10
	12	10	2	1	5	2	16	13	10	13
	12	5	1	0	4	5	14	14	13	6
	11	6	2	0	8	5	13	13	15	12
	12	8	2	2	8	9	11	16	13	11
	14	8	2	1	4	3	14	13	14	10
	13	6	2	0	4	3	16	8	14	17
	19	10	3	0	4	1	9	13	12	11
	12	3	2	0	5	5	14	10	13	16
	9	2	3	0	6	6	13	4	17	12
	14	4	2	1	5	5	13	7	9	13
	13	3	1	0	5	0	12	7	9	17
	12	6	2	0	6	7	12	9	7	11
	12	5	1	0	1	3	16	13	11	14
	12	9	6	2	6	3	11	14	13	13
	12	9	5	1	7	3	13	13	12	15
	9	5	1	0	6	6	15	13	12	14
	17	10	4	0	5	4	16	16	13	17
	11	4	4	1	10	2	16	13	12	12
	17	13	1	0	6	7	16	15	17	13
	11	4	1	0	5	8	15	13	15	9
	12	6	0	0	7	2	14	12	13	11
	11	4	1	1	4	5	15	12	13	11
	14	2	0	0	5	7	11	14	8	16
	11	5	1	0	6	6	11	13	8	15
	12	7	4	1	3	6	14	16	16	9
	8	5	4	0	4	7	11	7	10	14
	11	4	6	2	5	6	10	11	10	12
	9	3	1	0	7	5	11	7	13	14
	17	9	3	2	4	5	16	16	14	17
	16	12	2	2	6	1	13	16	13	12
	15	4	1	0	6	4	13	9	13	14
	11	7	1	0	7	2	15	10	12	7
	11	5	1	0	5	8	12	13	13	14
	11	6	2	1	1	8	12	8	14	15
	11	7	2	1	8	3	17	13	15	13
	13	5	2	0	8	5	14	15	13	10
	12	6	1	0	7	2	11	17	18	16
	12	6	2	0	3	7	17	10	16	12
	15	5	3	1	4	5	13	14	11	14
	12	6	1	0	4	7	11	13	8	14
	11	5	1	0	5	10	17	10	10	9
	11	6	1	0	5	5	16	12	13	15
	17	11	4	1	11	2	16	13	12	13
	15	9	3	1	4	4	10	11	13	12

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	O + (1)	O + (2)	O — (1)	O — (2)
RF	13	5	1	0	7	6	13	13	10	13
	10	5	3	0	8	6	11	14	14	11
	17	7	1	1	3	8	10	16	12	13
	14	6	3	0	6	3	13	12	13	13
	11	5	1	0	7	3	10	12	13	15
	12	4	2	0	6	5	18	12	12	13
	14	6	3	0	4	5	10	14	15	13
	11	6	3	2	8	5	11	11	9	13
	12	7	1	0	6	3	17	12	9	9
	13	5	1	0	9	5	17	15	13	14
	10	1	1	0	4	7	11	10	8	11
	12	5	3	0	4	8	17	13	9	16
	12	3	0	0	5	5	17	13	18	17
	10	4	3	0	7	5	11	15	15	11
	10	3	2	0	5	6	15	13	11	11
	13	8	1	1	5	5	16	10	11	10
	11	5	2	2	2	4	11	17	13	11
	14	4	0	0	6	5	16	12	10	10
	15	6	3	1	3	8	12	16	9	14
	11	6	8	4	6	3	11	15	13	17
	12	8	0	0	6	2	10	11	10	10
	9	5	2	0	5	6	11	9	10	13
	15	12	3	0	3	4	13	13	15	14
	12	6	1	0	6	7	17	11	13	15
	15	4	3	0	2	4	16	15	11	11
	14	3	1	1	4	5	13	9	12	13
	13	5	0	0	6	3	12	13	10	13
	10	5	2	1	7	4	13	10	11	9
	13	8	3	0	3	7	11	8	14	12
	14	4	2	1	8	7	12	13	13	17
	9	5	1	0	5	6	13	12	11	10
	11	5	0	0	6	9	15	10	12	10
	10	3	4	1	2	5	12	11	12	12
	14	6	5	0	9	1	12	14	12	11
	13	3	0	0	7	4	8	13	15	12
	14	9	2	0	6	6	9	11	15	14
	9	6	2	0	5	7	10	11	16	14
	12	5	2	1	3	3	9	16	16	12
	11	5	2	0	8	2	14	15	6	19
	12	9	2	0	5	4	17	15	16	10
	14	7	1	0	4	7	14	12	15	13
	12	3	3	1	3	6	12	12	15	10
	10	6	3	0	3	6	11	13	15	17
	12	9	1	0	3	7	11	16	17	12
	17	7	0	0	9	4	13	14	14	13
	15	11	4	3	4	4	12	10	12	15
	12	8	2	1	6	3	11	15	12	16
	13	7	3	1	4	4	13	12	9	11
	14	9	3	0	7	3	9	12	14	10
	10	7	1	0	9	4	14	14	15	12
	12	5	4	2	2	3	12	11	17	10
	10	7	1	0	4	5	9	10	12	8
	16	8	1	1	6	6	14	8	8	12
	12	5	4	1	5	8	12	14	14	12
	14	3	1	0	7	4	12	10	17	14
	14	8	3	0	6	10	8	13	15	14

Subject	Color	Suit	Number	Hit	ESP (1)	ESP (2)	O + (1)	O + (2)	O — (1)	O — (2)
RF	12	9	2	1	6	3	14	18	13	12
	11	5	2	0	9	5	12	14	14	8
	10	6	2	0	3	7	14	12	12	9
	14	5	2	1	7	5	12	10	15	16
	13	7	1	0	4	2	12	15	11	15
	9	5	2	0	4	4	9	16	11	11
	8	3	2	1	3	5	13	13	15	9
	14	10	6	4	5	5	15	17	14	14
	14	11	2	2	4	7	14	8	14	11
	10	4	1	1	2	2	14	12	9	9
	15	8	2	0	8	7	10	16	13	11
	8	3	1	0	5	9	9	10	12	10
	15	9	3	2	7	6	19	14	11	14
	12	6	2	0	6	6	9	9	9	7
	17	5	0	0	6	4	14	13	15	10
	12	11	2	0	7	7	10	13	13	7
	14	7	0	0	5	7	13	9	13	14
	13	7	1	0	4	5	14	11	14	12
	10	5	2	0	5	7	16	13	14	11
	12	5	2	0	4	7	13	11	12	14
	15	4	1	0	1	5	18	14	13	11
	9	5	2	0	3	7	13	8	13	11
	7	5	2	1	4	4	18	6	13	11
	17	5	4	0	5	6	17	15	13	10
	8	1	5	0	1	6	8	14	14	13
	15	6	1	0	4	5	16	9	9	15
	14	8	0	0	2	2	12	11	19	12
	15	9	0	0	11	7	13	14	8	9
	15	8	1	1	4	6	10	11	18	12
	16	9	3	0	8	7	13	14	11	20
	13	9	1	1	5	5	11	9	14	10
	12	7	1	0	3	6	14	19	14	13
	12	7	5	1	3	6	14	14	7	11
	8	3	4	0	6	4	13	13	15	12
	10	3	4	0	6	1	11	12	19	12
	14	6	1	1	5	8	15	13	12	9
	15	8	1	0	5	4	16	5	15	11
SF	11	5	1	0	5	7	16	14	12	11
	17	8	2	1	8	2	12	11	13	13
	11	7	2	0	4	4	11	10	15	9
	9	6	1	1	2	8	10	13	13	9
	18	10	2	0	4	5	13	14	17	12
	17	5	3	1	2	2	13	11	12	11
	17	6	2	0	4	3	11	15	15	16
	5	3	3	0	2	5	16	14	11	13
	12	7	1	0	3	5	11	14	13	12
	11	2	1	0	3	0	10	12	13	18
	12	5	2	0	3	6	18	15	14	11
Ga	11	5	1	1	8	2	14	12	14	13
	12	6	3	1	7	10	14	8	10	10
	10	7	1	1	2	5	13	12	11	14
	12	7	1	0	8	4	13	16	17	9
	13	6	3	1	7	9	15	12	12	13
	17	8	1	1	4	2	13	8	15	11
	11	9	0	0	6	5	12	11	12	13
	9	3	6	1	2	4	12	12	16	12
Ga	10	2	0	0	5	4	11	13	12	15
	15	8	1	0	3	7	11	12	20	14
	12	6	1	0	7	5	7	9	18	10
	12	4	1	0	4	4	11	13	12	9
	15	6	1	0	7	2	7	16	11	12
	10	5	4	1	7	5	13	13	11	9
	11	4	2	1	3	5	8	15	10	9
	15	5	0	0	4	5	14	11	14	10
	13	7	2	0	7	4	15	15	15	7
Ge	13	7	2	0	8	5	12	15	8	12
GG	13	8	2	0	4	6	16	10	8	12
	13	7	1	0	5	5	12	10	16	15
	13	6	1	0	4	8	11	11	9	12
	12	8	2	0	5	3	13	14	11	15
	13	9	2	0	6	7	13	11	13	17
	14	7	3	0	4	2	14	13	11	15
	10	6	1	0	3	4	18	13	12	13
JG	14	9	2	0	5	4	14	13	13	12
TG	13	8	0	0	8	7	13	12	14	14
Hi	16	8	3	2	7	4	10	13	12	9
CH	10	6	3	0	4	6	14	12	14	12
	13	6	1	0	4	6	11	12	12	16
	8	3	4	1	6	5	12	15	13	18
	15	5	2	1	2	9	10	13	9	13
MH	15	8	1	0	7	4	13	13	13	10
JH	12	6	1	1	1	5	13	15	14	16
	12	4	2	0	4	5	13	8	11	11
	10	6	1	1	6	3	13	12	12	12
SH	13	6	1	0	5	3	12	14	10	10
	16	4	3	0	2	7	12	14	13	13
	13	7	1	0	3	7	14	14	12	13
JJ	15	9	0	0	7	4	16	11	14	13
TJ	17	9	0	0	6	5	13	9	10	10
Ka	16	10	3	0	6	9	12	15	14	14
EK	18	7	1	0	7	8	10	12	13	10
	13	9	2	1	6	4	14	13	12	11
GK	15	7	5	1	4	5	16	11	14	13
WK	11	6	0	0	4	5	10	16	9	14
LL	14	3	0	0	8	3	11	12	10	11
ML	10	6	1	0	6	2	7	15	12	14
	13	10	3	1	6	6	11	11	11	17
	9	5	3	1	4	9	16	14	16	15
RL	14	6	2	0	4	4	15	11	13	6
	11	5	2	1	6	6	17	11	15	10
	13	7	1	1	4	7	9	10	11	12
	8	6	4	2	8	4	12	11	11	9
	16	6	1	0	7	6	11	10	11	12
	11	4	4	0	7	4	8	13	16	12
	15	9	0	0	1	3	16	11	13	10
Ma	15	9	3	2	3	2	12	11	11	13
Mi	16	10	3	0	5	2	14	13	15	13
Mis	10	3	2	0	6	3	19	11	8	9
	17	6	1	0	5	5	12	11	15	15
Mo	11	3	3	0	8	2	13	13	10	12
	13	7	2	1	4	9	11	6	12	16
	11	5	1	0	7	4	14	16	13	15

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	*O + (1)*	*O + (2)*	*O — (1)*	*O — (2)*
Mo	14	5	3	1	2	6	12	14	12	9
	13	4	1	0	2	6	10	13	12	11
	13	6	1	0	3	6	12	15	11	12
	10	3	1	0	4	5	14	9	17	12
CM	13	9	0	0	3	3	12	10	11	13
	14	5	1	0	4	7	11	7	14	10
	11	7	3	1	4	5	10	13	13	13
	13	4	1	0	4	6	13	12	11	16
	16	8	2	0	4	5	15	15	15	11
	17	8	3	2	6	1	12	7	15	12
GM	13	9	3	1	5	3	11	12	13	16
	11	6	1	0	7	4	15	16	12	9
	11	5	3	0	6	7	13	14	14	14
	14	8	2	0	0	6	10	14	16	10
	11	5	3	0	8	4	13	9	7	10
	12	3	1	0	6	8	15	13	15	13
	16	9	2	1	5	8	11	11	9	12
	15	7	1	1	2	7	11	16	12	10
	12	4	1	0	5	4	11	12	12	9
	12	7	2	0	4	7	12	7	17	11
	17	6	3	0	5	3	10	12	11	14
	18	8	3	2	7	5	13	18	18	16
	11	9	1	0	6	3	10	9	13	13
	14	4	0	0	5	7	14	13	13	9
	14	8	5	2	6	5	12	12	16	9
	14	6	1	1	6	3	18	10	11	12
	11	8	2	1	5	6	13	11	11	11
	13	7	3	1	8	4	12	14	16	16
	13	5	5	1	3	9	16	10	15	15
	12	4	2	1	8	4	11	13	14	10
	13	6	2	0	2	8	11	13	10	14
	13	6	3	1	4	3	14	12	11	7
	14	6	3	2	4	8	13	15	13	14
	11	6	3	0	6	4	11	11	9	18
	17	12	4	2	6	4	12	11	18	14
	11	5	5	1	5	6	13	10	13	17
	13	6	4	0	4	4	13	12	10	13
	13	6	2	0	4	10	15	15	9	14
	10	5	2	0	3	5	8	13	15	13
	10	5	0	0	7	4	11	13	10	9
	9	6	2	0	7	4	14	14	15	10
	8	4	4	0	3	5	11	13	11	5
	12	5	1	0	4	7	18	15	11	11
	15	7	1	0	7	6	10	12	13	12
	12	5	0	0	6	4	15	17	14	15
	11	4	2	0	7	4	14	10	14	15
	12	6	2	1	8	3	11	13	9	14
	13	8	1	1	4	5	16	10	12	11
	11	4	0	0	4	9	12	6	16	14
	15	7	4	1	6	4	8	18	15	12
	12	4	2	0	5	6	9	12	15	12
	12	7	1	0	2	6	13	9	11	10
	10	5	2	0	2	6	11	12	13	12
	13	7	1	0	4	3	15	12	19	17
	14	8	1	0	4	2	13	14	16	13
	16	8	1	0	6	5	12	12	11	12

Subject	*Color*	*Number*	*Suit*	*Hit*	*ESP (1)*	*ESP (2)*	*O + (1)*	*O + (2)*	*O — (1)*	*O — (2)*
GM	13	4	1	0	7	6	15	14	16	11
	13	7	2	0	8	4	11	10	12	11
	12	5	2	0	4	6	15	11	17	10
	16	10	2	0	6	8	13	15	11	12
	12	7	1	0	5	3	7	14	13	13
	15	8	3	2	3	0	16	12	14	14
	10	6	3	2	2	5	10	14	13	14
	11	6	1	0	3	7	9	7	15	14
	11	6	2	0	5	9	9	13	11	13
	12	6	1	1	7	5	13	13	11	8
	14	6	4	2	4	5	8	12	10	12
	13	8	4	1	6	2	15	13	11	12
	15	10	2	0	5	8	14	19	17	15
	16	11	2	1	3	6	13	12	13	9
	7	4	2	1	4	3	8	13	12	12
	13	9	2	0	8	3	11	10	11	13
	9	5	1	0	6	3	12	10	12	16
	11	5	2	0	4	7	15	6	9	11
	11	6	1	0	3	3	17	10	12	9
	12	6	2	0	7	3	13	14	11	11
	11	8	2	1	5	5	14	12	12	11
	6	5	2	0	4	4	15	10	15	10
	17	10	3	2	8	8	8	14	11	11
	12	6	5	1	4	6	10	13	14	12
	11	3	0	0	6	8	12	12	18	15
	14	11	1	1	4	6	13	12	14	14
	13	6	2	1	2	4	12	15	14	12
	9	4	1	0	5	4	10	10	14	17
	13	4	1	0	6	5	14	12	14	9
	8	5	0	0	4	7	14	10	10	13
	14	8	1	0	5	4	6	9	12	13
	15	8	2	2	3	7	14	13	11	12
	14	7	3	1	3	5	8	13	16	12
	16	11	5	3	3	5	13	10	12	10
	12	4	2	0	3	5	15	14	12	13
	11	7	1	0	2	6	10	10	13	15
	12	4	2	0	9	4	16	16	11	16
	13	7	2	1	6	2	10	11	13	13
	11	5	2	1	4	3	13	15	10	17
	11	6	1	1	7	5	15	12	11	16
	11	6	3	1	2	5	13	11	11	14
	12	6	2	0	10	4	10	12	14	14
	11	5	2	0	4	3	12	10	8	14
	9	5	2	0	6	5	8	11	11	12
	9	4	1	0	2	0	15	13	10	10
	12	6	3	2	6	4	13	14	11	20
	11	5	3	2	5	3	7	15	13	13
	12	9	4	2	4	1	13	16	16	12
	15	11	1	0	7	7	14	14	13	12
	11	6	2	0	2	4	12	12	12	11
	9	5	1	0	6	8	13	11	13	13
	12	6	1	0	5	5	12	16	15	11
	13	7	3	1	8	6	15	14	14	11
	18	9	2	0	8	7	10	15	14	16
	10	2	3	0	3	1	14	11	9	12
	12	8	1	1	6	3	14	12	14	10

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	O + (1)	O + (2)	O — (1)	O — (2)
GM	10	6	2	1	8	5	9	9	13	12
	16	8	4	3	3	2	14	12	14	14
	14	7	1	0	4	4	10	11	5	14
	18	9	2	1	5	1	15	16	8	14
	14	8	3	1	7	5	8	14	15	12
	10	5	0	0	4	6	12	12	12	15
	14	6	2	2	2	6	13	17	14	18
	10	4	4	1	8	6	12	12	12	12
	15	6	2	1	6	7	12	14	15	12
	11	4	2	0	3	4	11	7	10	17
	15	8	1	0	4	4	11	15	9	14
	8	4	1	0	3	4	10	13	14	14
	13	10	3	0	4	7	11	8	12	15
	13	6	4	1	7	4	10	14	8	13
	10	3	2	0	6	2	12	13	16	14
	9	4	4	0	4	5	15	12	10	13
	11	5	1	0	7	7	10	15	14	10
	9	3	0	0	7	2	15	19	18	11
	16	8	2	1	5	7	12	11	16	15
	18	11	0	0	6	1	11	14	15	11
	11	7	2	1	5	6	12	13	14	12
	10	4	1	0	6	3	15	12	13	13
	15	7	1	0	2	4	15	11	14	8
	15	11	1	0	3	8	11	9	19	8
	16	8	1	0	6	5	13	13	16	11
	14	9	3	3	4	7	12	14	11	15
	14	6	2	1	5	7	14	14	11	14
	13	10	2	1	6	3	14	10	15	12
	11	7	4	0	3	4	14	12	13	10
	9	4	2	0	3	4	13	11	13	13
	15	5	0	0	5	7	11	12	15	12
	9	5	2	0	6	6	12	16	11	16
	13	7	2	1	5	7	15	17	11	14
	8	4	0	0	4	4	12	11	11	10
	9	3	1	0	6	4	15	13	15	15
	11	6	3	1	6	5	15	13	13	11
	7	4	3	0	5	3	13	11	14	14
	11	4	3	1	6	5	14	12	11	15
	7	2	2	0	4	4	12	8	10	14
	14	5	1	0	8	0	17	14	14	13
	13	7	2	2	8	5	9	10	9	11
	9	3	2	0	4	2	15	13	15	7
	11	9	4	2	9	1	15	12	15	15
	8	4	1	1	2	9	11	11	8	10
	13	7	1	0	3	4	12	12	15	19
	11	6	2	1	5	6	15	13	13	14
	18	12	1	1	2	4	12	19	13	9
	10	5	5	0	3	5	11	12	12	14
	10	2	0	0	6	5	15	13	11	10
	13	9	2	0	7	4	13	14	11	19
	15	6	3	1	2	5	9	12	14	12
	9	7	4	1	4	3	8	13	15	10
	12	6	1	0	10	3	17	17	13	11
	13	4	1	0	3	9	17	12	13	9
	14	10	3	2	6	3	16	11	10	13
	7	2	3	0	4	7	13	15	9	12

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	O + (1)	O + (2)	O — (1)	O — (2)
GM	15	9	0	0	7	4	14	16	9	11
	11	7	1	0	3	3	9	6	13	13
	17	10	2	1	10	3	12	12	14	12
	10	7	0	0	4	9	12	9	10	13
	17	8	2	1	6	8	14	17	14	12
	14	8	2	0	6	4	9	17	12	14
	11	6	2	0	3	5	15	12	14	14
	10	6	1	0	4	7	15	12	15	14
	19	11	3	1	3	0	14	13	13	12
	10	4	1	1	7	3	13	10	13	9
	16	6	1	0	5	7	14	14	13	10
	17	10	4	2	8	2	5	11	15	16
	11	5	3	0	6	6	16	16	10	12
	15	7	1	1	7	9	9	16	14	12
	9	5	0	0	7	5	16	14	15	15
	14	6	3	1	6	6	7	11	16	13
	9	5	2	0	8	3	14	10	14	12
	15	8	2	1	4	3	16	10	17	12
	15	9	4	2	5	6	17	7	14	16
	15	6	2	1	6	4	10	10	17	15
	15	7	4	1	6	5	17	12	17	8
	12	8	2	2	5	6	8	15	6	13
	9	2	3	0	7	10	15	10	16	12
	14	7	3	1	1	3	15	14	18	19
	11	5	1	0	6	4	10	16	15	12
	14	8	3	1	7	6	11	15	12	16
	11	7	1	1	5	6	14	7	14	14
	13	7	0	0	3	5	13	13	9	13
	13	6	1	0	5	4	12	12	16	15
	11	7	1	0	6	1	15	12	12	15
	14	11	1	0	3	4	14	13	11	11
	15	8	3	0	5	7	13	10	13	13
	13	5	0	0	6	7	13	14	15	10
	10	3	4	1	3	4	12	7	13	15
	12	5	1	0	4	5	12	11	11	13
	9	4	2	0	2	6	14	9	15	13
	12	5	3	0	2	4	20	10	13	11
	13	7	3	2	8	6	10	15	7	18
	14	7	3	2	3	9	7	13	8	13
	11	6	1	1	4	4	12	10	11	13
	15	7	4	1	9	2	12	14	9	13
	17	7	1	0	3	4	14	12	14	13
	12	7	2	1	3	5	9	16	16	12
	12	6	3	0	5	5	12	14	16	13
	11	3	1	0	3	5	13	13	14	10
	12	8	3	1	3	7	14	8	10	13
	13	5	2	0	6	5	13	12	10	15
	11	5	1	0	5	5	15	7	12	15
	15	7	2	1	4	3	13	13	13	14
	11	5	1	1	7	5	12	8	12	11
	8	4	3	2	6	5	14	11	9	11
	13	10	0	0	4	3	12	12	14	18
	12	8	0	0	4	4	17	11	12	11
	14	4	1	0	5	5	12	11	12	14
	9	4	5	0	5	4	13	14	12	17
	11	7	2	0	4	3	12	13	9	15

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	*O + (1)*	*O + (2)*	*O — (1)*	*O — (2)*
GM	13	7	1	0	6	2	14	10	13	16
	14	10	2	0	5	6	12	7	13	13
	9	4	0	0	6	12	10	13	13	8
	15	8	3	0	5	4	12	10	12	17
	12	7	2	1	3	6	11	12	15	19
	11	5	2	0	3	5	15	8	11	11
	11	5	1	1	4	7	16	9	16	10
	15	4	2	0	5	7	12	13	11	12
	12	7	1	1	7	7	12	16	13	12
	15	5	1	1	4	7	11	15	9	11
	12	7	2	1	5	3	15	14	12	13
	11	4	1	1	3	3	16	13	6	13
	12	5	5	0	5	5	10	11	12	14
	11	4	3	1	7	6	13	15	10	13
	7	4	3	0	5	4	10	13	9	13
	9	5	1	0	3	3	12	9	9	14
	8	1	4	0	6	6	15	13	14	18
	13	8	6	2	2	2	11	8	12	12
HM	13	7	1	0	4	5	15	13	13	17
	14	7	2	1	5	3	8	10	12	10
	8	5	3	1	5	8	13	13	14	8
	16	4	2	1	6	2	13	17	10	14
	15	8	3	2	4	5	11	9	14	12
	11	7	2	0	6	6	13	11	12	14
	15	7	3	0	4	6	9	13	12	12
	14	8	0	0	7	2	14	11	12	12
	11	6	0	0	6	2	16	14	16	12
	15	10	1	0	1	3	10	12	13	10
	11	8	3	1	4	7	10	16	13	15
	8	4	1	0	4	4	9	16	12	14
	11	6	4	1	7	4	16	14	10	19
	13	8	2	1	7	10	12	12	6	16
	15	6	3	2	3	9	13	11	16	8
	14	6	2	1	10	10	7	16	15	12
	12	5	1	0	11	7	13	7	16	13
	10	7	3	0	3	5	16	10	15	12
	10	6	2	1	6	4	15	9	12	16
	18	6	3	1	11	6	16	14	16	14
	14	7	0	0	10	3	14	12	14	12
	14	9	1	0	8	7	9	15	6	14
	11	9	1	1	5	0	13	10	10	13
	15	8	0	0	10	4	11	11	10	14
	15	9	4	2	8	6	12	11	9	9
	11	4	5	0	10	4	16	10	11	11
	12	5	2	1	2	4	10	15	15	11
	11	7	4	2	10	2	15	9	12	16
	5	1	6	0	9	4	12	9	13	12
	9	5	1	1	9	4	18	15	12	14
	14	8	0	0	3	6	16	13	8	13
	14	7	2	1	6	7	13	16	16	16
	13	7	3	0	6	9	11	16	12	14
	9	4	2	1	3	9	12	10	15	15
LM	15	7	0	0	4	6	11	15	10	12
LBM	13	9	4	2	4	6	14	13	8	15
	11	5	3	1	5	7	12	10	17	12
	16	4	1	0	6	6	15	12	10	16

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	*O + (1)*	*O + (2)*	*O — (1)*	*O — (2)*
Pi	13	8	2	1	9	8	13	10	12	15
Pr	11	6	3	1	6	10	13	11	10	14
	10	2	1	0	4	4	14	12	15	12
	11	6	3	2	4	6	9	13	12	14
	11	3	1	0	8	5	20	13	12	15
	11	7	1	1	4	3	12	12	13	13
	13	8	0	0	9	4	13	13	15	8
HP	12	9	2	0	8	7	10	13	18	12
	15	8	2	1	5	5	14	16	15	13
	9	2	4	0	3	10	13	7	12	12
	13	6	2	0	3	1	15	12	10	10
	15	5	1	0	6	8	13	13	15	14
JP	14	8	2	0	7	3	13	11	12	6
	10	7	4	0	7	1	16	14	15	11
	17	15	1	1	4	3	7	9	12	13
	14	7	3	0	5	4	14	12	12	12
	17	14	1	0	6	7	12	17	19	11
MP	16	8	1	1	4	4	14	16	14	10
	14	8	3	0	6	6	11	12	17	10
	16	8	3	1	2	5	13	16	14	11
	14	6	1	0	5	4	14	12	16	13
	18	11	2	1	3	5	11	12	13	13
	18	10	1	1	6	0	13	15	10	13
	14	6	4	1	2	4	16	13	14	9
	9	4	0	0	6	5	10	12	13	12
	11	5	2	1	5	7	11	15	13	10
	13	6	5	3	5	6	9	11	12	15
	13	5	4	1	4	4	13	15	12	14
	11	7	0	0	3	3	12	15	14	12
	15	6	2	0	4	6	11	15	15	8
	11	8	1	1	6	4	11	17	18	11
	11	8	1	0	10	3	16	14	14	16
	19	14	1	1	10	4	14	18	15	21
	9	7	3	0	3	4	10	17	11	12
	16	5	2	0	4	2	13	16	15	11
	13	6	0	0	6	3	14	13	16	16
	12	5	0	0	5	7	14	16	15	10
	12	7	0	0	5	2	14	14	10	11
	16	10	2	0	10	8	15	16	8	13
	10	5	3	0	3	6	14	14	11	13
	11	5	0	0	6	7	13	13	16	12
	9	4	4	0	8	4	14	17	14	10
	13	7	1	1	7	9	10	15	13	12
	10	2	2	0	4	8	14	9	11	14
	10	5	2	0	4	4	11	11	16	17
	13	5	0	0	5	3	14	12	15	12
	13	8	2	2	6	3	14	17	14	13
	7	1	1	0	4	8	11	12	15	13
	7	4	3	0	5	6	9	14	13	11
	10	5	0	0	8	3	12	11	10	14
	12	6	2	1	6	6	11	12	12	12
	7	4	2	0	10	5	12	15	17	14
	13	5	4	0	6	4	12	11	17	17
	9	4	1	0	6	4	11	14	10	12
	14	11	2	0	5	3	12	18	12	13
	18	10	2	0	9	8	13	13	15	12

Subject	Color	Suit	Number	Hit	ESP (1)	ESP (2)	O + (1)	O + (2)	O — (1)	O — (2)
MP	14	4	3	0	5	10	12	9	14	8
	11	7	2	0	4	3	8	14	18	10
	9	3	3	0	1	10	12	14	14	9
	15	4	5	1	4	2	13	14	11	7
	14	7	2	0	7	5	7	8	13	11
	13	7	0	0	7	6	15	10	16	9
	10	3	1	0	7	4	9	10	9	9
	13	6	2	2	7	5	10	15	13	15
	12	8	2	1	6	3	16	9	14	15
	13	5	2	0	4	4	14	11	15	11
	6	3	4	2	5	2	11	14	10	12
	15	7	2	2	5	9	13	11	14	16
	12	4	3	0	5	4	14	11	11	12
	15	8	6	3	7	6	13	12	12	15
	14	6	1	0	7	6	9	8	12	10
	13	7	4	1	4	4	16	9	10	14
	14	3	3	0	4	1	9	8	14	13
	14	4	2	1	4	3	14	16	15	13
	13	9	0	0	6	5	11	12	14	9
	9	3	4	0	5	8	11	12	13	11
	13	7	3	2	7	4	12	11	15	14
	15	10	0	0	5	5	12	18	11	12
	12	6	1	1	3	1	17	12	13	13
	14	8	5	4	4	8	10	15	12	10
	13	5	3	2	5	5	17	15	16	9
	19	10	1	1	3	3	16	11	12	14
	9	5	2	0	5	2	10	17	8	12
	11	5	3	1	5	6	13	15	15	14
	12	4	0	0	2	3	15	15	15	11
	12	5	2	0	6	5	9	16	16	8
Re	12	5	1	0	6	6	15	9	14	11
	11	7	1	0	1	1	15	13	12	12
	13	6	2	1	4	5	6	13	11	19
	19	7	3	0	2	5	9	16	13	12
	12	4	3	2	3	6	15	11	10	14
	16	9	3	2	7	4	12	14	15	15
	11	5	1	0	5	6	8	9	19	14
	17	11	3	1	8	2	11	12	13	10
	9	6	0	0	5	8	11	16	13	7
	15	8	5	3	6	5	16	15	13	13
	11	6	0	0	8	5	11	13	9	14
	13	8	0	0	5	5	12	10	16	12
	9	3	1	0	4	3	12	10	12	9
	17	13	3	2	4	5	14	9	12	15
	9	7	0	0	4	3	12	10	10	13
	8	4	1	0	7	9	14	14	15	11
	12	5	2	0	7	7	12	15	13	12
BR	12	6	3	0	2	4	10	13	9	11
	12	4	1	0	3	4	11	9	13	14
	13	2	3	0	4	10	11	14	14	14
HR	14	7	1	0	6	2	12	16	10	7
	13	8	1	0	6	4	10	10	14	13
	12	7	5	3	3	5	13	14	12	14
LR	12	3	3	0	3	6	15	11	13	10
	13	6	6	1	3	6	12	13	13	20
	13	9	4	1	4	7	12	14	10	15

Subject	Color	Suit	Number	Hit	ESP (1)	ESP (2)	O + (1)	O + (2)	O — (1)	O — (2)
LR	15	9	1	0	4	6	15	12	9	12
MR	10	3	1	0	10	9	15	19	16	14
	16	7	0	0	5	8	7	15	17	11
	6	3	1	0	9	6	13	11	13	12
	13	6	2	0	7	4	11	11	10	9
	10	6	0	0	3	5	10	11	17	10
	8	4	1	0	8	5	13	12	15	13
	12	5	1	0	1	3	13	11	10	15
	9	6	2	1	7	5	13	14	12	11
	14	8	4	1	4	4	12	9	7	9
	15	8	0	0	5	10	13	12	13	16
	8	2	2	0	10	2	11	14	12	15
	11	5	3	1	5	3	11	13	8	10
	12	6	0	0	5	5	10	15	15	12
	11	4	2	0	7	4	13	16	14	11
	11	8	1	1	8	5	11	11	12	15
PR	12	5	2	0	2	9	11	17	12	15
	12	7	2	1	8	2	14	14	10	6
	13	7	1	0	4	5	14	9	11	12
	16	6	4	0	4	2	13	11	11	14
RR	11	7	0	0	7	7	11	9	15	12
	12	6	3	1	4	5	14	14	16	11
WR	13	8	4	1	12	6	5	8	14	12
	11	5	2	0	5	2	15	16	11	12
	10	3	1	0	6	7	13	15	12	16
	5	2	0	0	4	5	10	11	10	12
	10	6	0	0	3	3	11	14	9	10
Sc	10	4	1	0	5	7	15	11	11	12
DS	8	4	3	1	8	7	12	10	17	14
	14	7	4	1	6	5	15	16	13	12
	8	3	2	0	3	3	15	13	13	15
DJS	11	7	4	2	2	9	14	16	10	9
	12	7	0	0	8	5	12	14	10	9
	13	6	2	0	3	6	14	10	12	14
	9	4	2	0	3	5	13	10	17	12
	14	5	2	1	6	4	13	10	12	12
	13	7	2	0	5	3	13	12	14	10
	6	4	2	0	5	4	10	9	11	16
RS	11	7	4	2	5	1	11	14	12	12
	12	5	4	1	8	6	16	12	13	11
	13	8	2	0	5	6	8	11	11	12
	16	7	0	0	2	4	7	18	13	13
	13	7	0	0	2	9	15	13	12	8
	14	9	0	0	4	3	15	16	13	15
	11	8	0	0	2	3	11	9	14	10
	15	8	1	0	3	3	15	15	16	19
SS	10	4	0	0	3	7	11	12	7	14
	15	8	3	1	5	7	11	6	7	11
	13	6	1	1	3	5	7	5	13	14
	16	7	2	0	4	6	13	15	12	19
ET	13	4	1	0	9	5	13	14	11	9
	14	7	0	0	4	4	13	13	15	17
	15	5	2	0	6	3	12	11	15	13
	11	8	2	0	6	4	13	12	11	10
	8	2	1	0	6	4	10	14	12	16
	15	6	0	0	5	3	13	17	13	16

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	*O + (1)*	*O + (2)*	*O — (1)*	*O — (2)*
ET	15	8	2	0	4	5	12	15	6	13
	14	10	0	0	4	4	10	17	10	16
	13	4	1	0	6	7	14	13	16	13
	14	7	3	1	5	6	10	13	5	14
	16	10	1	1	5	4	14	14	14	15
	11	5	2	0	4	5	13	11	13	15
	11	5	1	0	4	6	11	12	9	15
	14	8	1	0	2	7	11	14	13	14
	12	4	2	0	4	4	15	14	14	14
	12	7	1	1	6	5	17	13	13	16
	15	8	0	0	1	7	15	12	14	13
	12	7	2	0	6	3	9	15	15	16
	12	8	3	2	6	3	14	11	11	14
	12	4	1	0	4	5	10	11	16	10
	10	4	3	0	6	4	10	11	14	9
	17	9	0	0	4	7	13	14	14	14
	18	10	3	1	3	5	13	14	13	9
	13	8	2	0	5	8	13	11	12	14
	11	3	5	1	6	6	14	13	13	14
	16	5	1	0	10	8	11	14	13	11
	13	6	1	1	9	2	12	14	14	12
	17	8	3	1	6	5	12	10	16	11
	13	5	3	0	8	6	15	13	8	9
	13	9	2	1	5	4	9	11	14	10
	14	4	3	1	7	4	9	11	13	10
	12	5	0	0	4	1	11	13	11	14
	12	5	0	0	4	4	9	16	14	11
	16	6	1	0	5	5	13	8	12	8
	14	8	1	1	8	1	12	14	11	11
	14	8	1	0	3	8	12	14	12	17
	10	3	1	0	7	4	8	16	11	9
	10	2	3	0	4	7	9	11	13	16
	13	3	2	0	7	5	10	13	11	9
	14	9	0	0	5	8	12	13	14	10
	7	2	2	0	6	8	9	7	13	13
	9	4	0	0	6	9	12	9	15	13
	11	7	3	2	3	5	12	11	13	12
	12	7	0	0	4	3	13	12	14	8
	8	6	5	1	5	5	11	11	15	10
	11	4	1	0	3	5	11	14	9	16
	17	14	1	1	3	4	13	11	15	10
	15	9	2	2	5	5	13	13	13	15
	13	9	2	1	6	8	10	16	11	15
	11	4	3	0	2	9	16	13	9	16
	15	5	1	0	6	5	13	12	13	15
	12	10	0	0	7	5	13	16	12	13
	13	10	1	0	6	4	10	14	11	17
	10	6	5	1	6	5	10	10	15	14
	13	4	2	0	4	6	11	15	8	13
	15	9	7	2	6	7	13	10	9	14
	14	4	3	1	3	4	11	11	14	11
	15	11	3	1	1	3	12	10	13	13
	12	4	0	0	5	5	17	11	14	11
	12	6	4	0	5	3	13	12	11	14
	14	10	0	0	8	4	13	14	15	17
	14	7	2	1	5	5	16	9	14	16

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	*O + (1)*	*O + (2)*	*O — (1)*	*O — (2)*
ET	10	7	2	1	7	7	11	13	14	10
	12	9	2	1	3	2	13	12	9	12
	14	5	3	0	0	4	10	14	13	13
	10	5	1	0	7	5	10	10	13	13
	8	3	0	0	2	4	14	16	12	13
	17	8	3	1	7	5	9	13	15	12
	13	4	2	0	7	7	18	17	11	12
	9	3	1	0	1	4	15	10	12	16
	16	10	3	1	2	3	15	13	10	8
	12	6	3	1	5	7	13	12	12	13
	14	4	6	1	7	7	14	14	9	9
	15	7	1	0	9	5	18	13	12	7
	15	9	0	0	5	5	12	13	15	15
	11	2	0	0	6	3	13	12	8	11
	11	5	2	1	4	3	15	14	10	8
	12	4	1	1	6	7	14	16	14	18
	12	4	1	1	5	5	12	12	8	9
	14	5	3	2	5	5	19	12	14	15
	13	7	4	1	4	6	10	10	13	15
	11	4	3	1	3	5	13	11	9	10
	14	8	2	0	4	8	15	14	12	13
	11	2	2	0	5	5	13	9	13	16
	9	5	0	0	4	5	10	9	14	16
	12	7	0	0	5	1	12	11	11	14
	9	4	2	1	5	6	15	13	13	13
	16	6	1	0	2	7	13	17	11	17
	14	8	0	0	5	6	13	8	9	12
	16	5	4	1	3	4	13	13	11	12
	16	8	3	0	12	7	13	11	14	11
	14	7	3	2	6	3	13	14	14	11
	11	3	6	1	3	7	9	11	13	10
	15	10	4	1	6	11	18	14	9	8
	10	8	1	0	5	6	18	11	14	14
	9	3	2	1	5	4	15	17	13	13
	11	6	2	0	6	9	16	12	12	11
	13	7	1	1	1	5	12	20	14	15
	10	6	3	1	3	2	11	11	13	15
	16	8	1	1	2	9	11	11	12	18
	13	6	2	2	5	5	10	12	8	12
	11	5	3	1	4	5	13	15	11	11
ERT	11	5	5	2	6	4	12	18	5	10
	12	7	1	0	7	5	12	13	14	13
	16	10	0	0	4	6	15	14	16	12
	14	6	0	0	3	3	15	11	12	14
	12	8	2	0	8	6	12	15	11	10
	13	7	1	0	4	10	11	14	13	10
	18	10	2	2	4	3	5	12	11	14
	10	6	1	1	9	3	8	8	14	10
	11	6	1	0	3	6	14	15	8	11
	13	7	1	0	6	2	12	13	11	14
	13	7	0	0	7	7	14	11	18	9
	7	2	0	0	4	4	14	16	12	12
	14	7	1	1	5	5	13	15	13	14
	12	6	2	0	4	7	11	18	13	13
	12	4	2	1	6	2	15	7	11	13
	12	7	2	0	2	5	10	8	10	15

Subject	Color	Suit	Number	Hit	ESP (1)	ESP (2)	O + (1)	O + (2)	O − (1)	O − (2)
ERT	10	3	1	0	3	6	11	8	15	13
	14	6	4	2	8	7	13	13	16	11
	14	8	2	0	8	3	13	12	11	15
	8	4	4	0	5	9	15	11	14	13
	14	8	1	1	3	9	14	10	12	10
	8	5	6	1	3	5	13	15	7	14
	11	4	1	1	3	6	9	14	16	12
	18	7	5	2	2	3	11	13	15	15
	13	6	2	1	4	3	16	9	10	10
	12	6	1	0	3	2	18	8	16	8
	14	6	3	1	4	2	14	18	8	13
	9	4	3	0	4	6	13	14	13	15
	17	10	3	0	6	4	11	13	16	15
	10	5	1	0	4	7	8	11	15	9
	15	7	2	0	4	5	12	8	10	12
	12	4	1	0	5	4	14	14	13	11
	15	6	2	1	5	5	15	13	15	12
	6	1	2	0	6	2	13	10	13	15
	14	6	0	0	5	3	11	9	13	14
	10	4	1	1	4	2	15	12	7	17
	12	4	3	0	6	7	12	13	11	13
	10	8	2	1	3	4	10	9	14	8
	10	3	0	0	5	3	12	14	12	13
	13	9	2	0	4	4	11	14	17	17
	9	7	0	0	4	6	14	10	10	14
	12	3	1	0	5	5	14	14	17	10
	13	10	1	0	4	5	12	11	14	7
	12	4	0	0	2	2	16	12	11	12
	13	6	3	0	4	11	14	10	10	14
	12	5	2	0	4	6	13	11	16	13
	11	6	1	1	8	4	9	14	15	9
	13	6	1	1	9	7	5	14	13	12
	14	11	4	1	6	3	14	14	11	10
	13	9	3	2	8	4	14	11	14	15
	14	8	2	0	9	3	10	13	10	12
	11	4	0	0	3	5	11	13	14	17
	8	2	1	1	4	7	14	13	15	10
	13	6	3	1	7	3	12	13	11	14
	11	4	1	0	3	2	14	13	16	11
	11	5	2	0	5	4	14	10	13	9
	11	5	1	0	4	8	12	13	13	13
	13	6	2	1	7	5	14	13	10	9
	12	9	3	1	4	2	17	12	16	14
	16	5	1	0	3	5	19	12	11	11
	7	6	2	0	7	4	9	11	10	8
	11	5	1	0	3	6	15	17	13	13
MT	11	6	5	1	3	4	14	14	14	15
	17	6	1	0	7	3	9	15	15	18
RT	9	3	1	0	3	4	15	9	15	7
ST	18	14	1	0	4	3	15	11	11	18
TT	16	9	1	0	6	4	9	14	12	11
	10	5	0	0	9	2	12	14	13	11
	13	7	2	1	3	7	4	14	15	13
	13	8	2	1	6	4	11	12	9	12
	12	2	1	0	3	5	9	14	15	13
	15	7	0	0	7	8	18	14	14	14
TT	14	7	5	2	3	6	11	15	13	14
BV	12	4	2	1	6	5	17	9	12	12
	13	7	0	0	2	3	13	13	11	13
	6	4	3	1	6	6	18	11	17	10
	10	9	0	0	7	4	14	9	11	9
	10	5	0	0	1	3	13	14	17	8
	12	7	4	0	5	3	13	11	10	11
	8	3	4	0	2	5	10	13	13	13
	14	5	3	0	2	2	18	15	15	14
	15	8	1	0	5	7	10	15	9	9
	14	5	2	1	4	8	17	10	12	15
	12	6	1	0	3	5	11	7	14	13
	14	6	3	2	7	4	12	12	10	13
	14	8	1	0	2	4	11	12	9	12
	12	9	3	1	8	5	13	15	9	9
	15	8	2	0	6	5	17	13	14	9
	9	6	4	0	7	2	10	12	15	13
	10	1	1	0	6	5	14	12	19	11
	13	7	4	1	3	2	12	13	13	15
	12	5	2	0	7	4	10	12	12	16
	14	7	0	0	7	3	13	17	9	11
	11	3	2	0	4	10	11	10	9	16
	12	4	3	0	5	3	16	11	15	16
	9	5	1	0	7	3	15	8	14	12
	14	7	3	1	3	6	8	9	16	12
	8	4	1	0	4	3	11	15	13	11
	16	8	2	1	3	2	13	14	12	12
	11	5	1	0	5	4	15	15	16	12
	7	4	1	1	6	5	19	13	11	10
	7	5	5	0	7	4	12	17	12	9
	16	6	2	0	4	3	14	15	15	7
	13	5	1	0	3	10	13	12	11	14
	14	3	1	0	5	4	11	10	13	16
We	13	4	5	1	5	6	15	12	14	14
AW	12	4	1	0	4	3	12	14	16	14
	16	9	3	0	8	5	12	11	12	18
	14	6	4	1	4	5	12	13	9	13
	9	4	2	0	8	3	12	14	12	16
	11	8	2	1	6	3	13	14	15	15
	10	6	3	0	6	7	15	15	11	10
	17	10	4	2	5	4	12	15	12	5
	13	6	0	0	2	3	17	15	13	13
	10	3	2	0	3	6	11	8	12	15
	12	4	0	0	6	4	14	8	13	11
	13	7	1	0	1	10	12	13	12	11
	12	3	1	0	7	5	12	11	10	9
	14	10	4	2	6	2	16	13	16	12
	11	8	2	1	2	4	13	15	13	11
	16	10	2	0	4	5	12	12	18	14
	13	7	3	0	3	4	13	9	14	12
	11	3	0	0	3	4	10	13	15	11
	13	8	1	0	7	2	9	13	10	15
	9	4	1	0	3	9	11	8	15	9
	7	2	3	0	4	4	9	11	14	12
	12	9	0	0	6	7	10	13	14	16
	11	7	1	0	3	8	14	14	12	13

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	O + (1)	O + (2)	O — (1)	O — (2)
AW	14	9	1	1	4	8	15	13	11	10
	15	4	1	0	8	2	12	12	11	14
	11	4	3	0	7	7	15	15	11	12
	13	5	1	0	10	7	13	12	15	10
	8	4	1	0	3	5	16	13	11	10
	8	4	1	0	2	3	10	16	13	11
	12	6	3	1	6	5	9	15	17	14
	10	7	1	0	0	6	10	16	16	12
	10	7	1	0	11	3	11	15	7	14
	11	6	2	0	4	4	15	13	14	16
	15	9	1	1	3	6	14	12	10	13
	9	4	1	0	4	9	14	12	12	8
	12	5	0	0	8	4	15	17	10	13
	13	2	2	0	6	11	17	12	11	13
	11	8	2	1	6	5	13	14	10	10
	13	6	0	0	6	3	16	12	12	10
	12	4	4	0	4	1	10	15	12	12
	13	8	4	0	6	8	11	10	18	10
	12	8	2	2	4	3	15	11	14	13
	16	7	2	1	10	4	11	12	12	13
	17	8	1	0	4	5	15	11	14	13
	13	8	4	3	8	2	13	12	10	11

Subject	*Color*	*Suit*	*Number*	*Hit*	*ESP (1)*	*ESP (2)*	O + (1)	O + (2)	O — (1)	O — (2)
AW	8	2	0	0	6	6	9	10	14	11
	14	6	3	1	9	9	13	15	12	13
	9	6	1	0	3	9	12	15	11	14
	12	6	2	1	6	3	12	9	11	12
	11	8	1	0	6	3	9	12	14	11
	16	4	3	1	7	6	8	13	16	10
	12	5	2	0	5	8	11	10	19	14
	15	6	3	1	5	4	18	13	11	14
CW	9	5	1	0	3	7	16	9	15	12
	10	1	1	0	5	2	10	10	9	10
	13	3	4	2	7	3	14	18	11	14
	9	6	2	0	3	5	14	12	9	11
	13	7	2	0	5	6	13	14	16	13
	12	4	1	0	2	4	10	14	9	11
LW	15	4	1	0	6	6	14	14	12	11
	9	7	5	3	4	4	11	14	14	12
	17	6	1	0	6	3	16	16	12	14
	14	6	3	1	6	4	10	11	13	14
	11	2	1	0	8	7	12	10	11	12
	16	6	1	0	7	5	8	12	8	11
	16	11	1	1	2	5	11	12	13	17
	14	6	2	1	4	1	10	14	10	11

FURTHER DATA FROM A CASE OF HIGH SCORES IN CARD GUESSING

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NUMEROUS REQUESTS for the complete records and for a more adequate description of techniques have been received by the author since the publication of the original data (3). Kennedy (1, p. 95) has suggested that in view of the extremely high scores "full determination of the conditions under which the unusual results occur would seem to be of paramount importance." In the same citation, it is stated that "no information as to when and how the check-up for number of hits was made is included in the experimental report." It is the author's hope that some of these queries and criticisms will be answered in the present paper.

It will be recalled that the subject, in a series of 1,850 trials made 1,349 correct guesses at the symbols on the usual pack of ESP cards. The conditions under which the experiment took place were as follows: The experimenter seated at his desk exposed, at 9:00 p.m., the top card from the deck which had just been manually shuffled and cut. This card was placed face up to one side of the deck which was kept face down. At 9:01 the second card was similarly exposed. Immediately after exposure of each card, its symbol was written in the card column of a previously prepared mimeographed record sheet. In this fashion 50 trials or two shufflings of the pack were run during each of the experimental sessions. Meanwhile the subject at her home, separated from the experimenter by approximately one-quarter mile, wrote her guesses in the call columns of her record sheets. The first guess was made at 9:01, the second one minute later. During the day following the session, the subject's records were mailed or delivered to the experimenter. The card records were kept in a desk drawer. As a rule the sheets were compared during the week-end. Checking was done by noting correct and incorrect responses on the experimenter's record sheets. Rechecking was performed on the subject's sheets. No witness was present during the checking, nor were the card records locked up during the day. This constitutes a serious criticism to which the author can only respond by

stating that to the best of his knowledge no one at his home interfered with the records in any way. Furthermore, the sheets show no erasures and are all in the same handwriting. Photostatic copies of the originals are available for inspection if desired. The complete call and card records for the 1850 guesses are given in the appendix, arranged in two columns for each of the 74 trials. The upper line in each trial contains the records made by the subject and the lower line the experimenter's records, calls and cards, respectively. Correct guesses have been indicated by upper case letters in the call line. The division into units of five has been made for legibility. There was no experimental hiatus between successive fives. The symbols on the cards are represented in the table by the following letters: star, s; cross, p; waves, w; square, r; circle, c.

Lund (2) has recently published data on the attention value of the various symbols. He found that the star had twice the attention value of either the cross or the rectangle. Other writers have mentioned the possibility of symbol predilection. When the data of the 1,850 trials are studied, it is seen that the star was used by the subject 366 times, the square 367 times, the circle and the waves each 372 times and the cross 373 times. These differences are obviously not significant.

Among the questions frequently asked in ESP work is whether the subjects use any system of guessing. It would seem apparent that a sophisticated subject would try to use each of the five symbols five times in each trial. This has, however, been found not to be the case in long continued trials. Of the 74 trials in the experiment, only 19 contained calls in which each symbol was mentioned 5 times. Ten of these 19 occurred at the beginning of the experiment. It seems possible to conjecture that the subject gave up systematic guessing after the initial period of the experiment. Table I gives the distribution of the frequency of occurrence of each symbol within a trial. The table is to be read as follows: There was one trial in which only one guess was given for a square (R). There was one trial in which only two squares were given as responses.

TABLE I

Frequency of Occurrence of Symbols within a Trial

No. of symbols in a trial	Frequency				
	R	P	C	W	S
1	1	0	0	0	0
2	1	2	0	0	0
3	2	3	5	2	6
4	12	12	11	15	16
5	41	40	41	41	33
6	15	10	11	11	16
7	1	5	6	5	2
8	1	1	0	0	0
9	0	1	0	0	1

In accordance with common test procedure, the consistency of the subject's scores was determined. An odd-even reliability coefficient of .74 uncorrected was obtained for the subject's calls. When the Spearman-Brown formula was applied, this rose to .85. This indicates high internal consistency of response and seems to show that the factors making for high success in card guessing were present throughout the entire 74 trials.

In conclusion, study of the data reveals that, over the whole series, the symbols had approximately the same attention value or, expressed in other words, the subject had no definite symbol predilection. The reliability of performance in this task was high as indicated by an odd-even reliability coefficient of .85.

REFERENCES

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2. LUND, F. H. Extra-Sensory Perception Another Name for Free Association? *J. of Gen. Psychol.*, 1939, **20**, 235-238.
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APPENDIX

COMPLETE DATA FOR SERIES A, SUBJECT S

1.	c p R p c w s r c w	w p C C s p w c c w	w s w r R s r r s r	c p s w r p c w c p	w s s P r s r p p s
2.	p W W w s s w w c c	r p c p c w r r w r	s S w w r w s p r s	s P r r s p p s c c	R c c P C r p s p c
3.	c s R W C s c r w c	s R W C r w r w c p	P R W r p p r w c s	p w c S p w p p s c	p s w c s s r p s r
4.	s R p R c p r c r p	P p R C p p s r c s	w s s R C s p w r c	w s S W R p w s w r	w c C p W c s c w w
5.	C s w p S c c p s s	C W R R c c w r r r	R S C P S r s c p s	s R W w w w r w c s	P p c r P p w p w p
6.	P R r p p p r s w c	S c c S s s p p s w	p w w C R c c r c r	c s w s r p w s w w	C w w p R c r p s r
7.	s C C p S r c c c s	P W w C p p w p c w	c p R S r p s r s p	S W R C R s w r c r	p S W R w w s w r p
8.	C r p p R c c s w r	P c s s P p w r w p	C r s w w c c p s s	S c W P w s r w p p	R S W C R r s w c r
9.	C S R C P c s r c p	W R S C W w r s c w	P w S W r p r s w s	C r w R c c w p r r	w s P S P c w p s p
10.	C C W R W c c w r w	S P P C R s p p c r	w W S r R r w s w r	P S C S R p s c s r	P S p W c p s c w p
11.	P r r w W p p w r w	c C W C r r c w c c	P p r c R p s p w r	c p R s s s s r c p	R p W S r r s w s c
12.	c r W P s w p w p w	W C s P R w c c p r	p w S P S c r s p s	P R w c S p r c s s	R R p w W r r s c w
13.	S P s w W s p r p w	P S W W c p s w w w	R C p w S r c w p s	p C R R C c c r r c	S P R S C s p r s c

14.	R R P C W r r p c w	W S P C C w s p c c	W w W R P w c w r p	S S S R W s s s r w	C P R S P c p r s p
15.	S W R W r s w r w w	R W p S R r w w s r	c C C C R p c c c r	P P s P P p p c p p	S S C R S s s c r s
16.	P P R S W p p r s w	S R R R C s r r r c	w C S P C s c s p c	c S W P R p s w p r	W w C W c w c c w w
17.	P R W W W p r w w w	S s c s S s c s p s	W R W R P w r w r p	S P C R C s p c r c	R P S C C r p s c c
18.	W P P W R w p p w r	S C C S S s c c s s	R S P c C r s p r c	R P C S W r p c s w	R P W C W r p w c w
19.	C W S S R c w s s r	P C C R C p c c r c	W P W P S w p w p s	C P R R S c p r r s	P S R W W p s r w w
20.	P P r P R p p w p r	S W C W R s w c w r	S C P S S s c p s s	C W C S R c w c s r	R W R P C r w r p c
21.	C P R S W c p r s w	W R W R P w r w r p	C S W w R c s w s r	s C S C W p c s c w	R P s C c r p p c s
22.	s C S P C w c s p c	W S C C R w s c c r	W P P p S w p p r s	R s S rW r p s p w	C S W R p c s w r r
23.	W p s S C w c r s c	W r C c P w w c s p	P R W C R p r w c r	S S P W R s s p w r	P p R C w p s r c p
24.	S P R s C s p r p c	S r S P C s c s p c	C P W W r c p w w c	c W S S p r w s s w	R R W R P r r w r p
25.	C C W R s c c w r r	s W c W P w w p w p	R r S P C r s s p c	R C w S P r c r s p	P r rC S p w s c s
26	P rW S s p s w s r	P R S w c p r s p w	r c C w R c r c s r	C P c C c c p p c w	R pW W p r s w w c
27.	r p S s C p w s p c	c R W s S r r w r s	c S S P W r s s p w	P C p p P p c c r p	w C W C c s c w c w
28.	p p c R w c w w r r	C r R s S c s r p s	pW W C P s w w c p	R P c C p r p p c s	W S P R w w s p r c
29.	W s S S S w c s s s	R pW c C r c w s c	R s r r R r w p p r	c p P C S r p p c s	R P p wW r p w c w
30.	W p w w P w c c c p	P W c c p p w r s r	s w p C P w c s c p	c S p P c r s w p r	R P S wW r p s s w
31.	c P p R S r p c r s	W P w P W w p s p w	R R W P C r r w p c	S R C S c s r c s w	p C S rC w c s p c
32.	W W C P w w w c p p	S S R R P s s r r p	P C P W S p c p w s	S R R c C s r r r c	S p CW p s w c w c
33.	P W C W C p w c w c	P C W C R p c w c r	R S P W R r s p w r	s S s S P c s p s p	R S R cW r s r s w
34.	S R S R P s r s r p	P S S wW p s s c w	R C W R C r c w r c	S W P C C s w p c c	P W W R P p w w r p
35.	c sW S C r w w s c	r C C R W s c c r w	S R R P W s r r p w	C C w S c c c p s p	P R S W P p r s w p
36.	P r S R C p p s r c	c S P W R r s p w r	C w P W S c s p w s	W p S p C w c s r c	c r R W P w c r w p
37.	C P R W S c p r w s	R r R W S r p r w s	P R P S C p r p s c	R P C C W r p c c w	s S w W S w s c w s
38.	S S P W W s s p w w	C p P R S c s p r s	R S C w R r s c c r	pW P wW c w p r w	P P R C W p p r c w
39.	W W w C r w w s c c	P W s C P p w r c p	R P S c P r p s s p	S S c R W s s w r w	C c C P R c r c p r
40.	w S C P C s s c p c	R R s w r r r c r c	P cW S c p w w s w	C R P P R c r p p r	W W P S S w w p s s

41. R P S R s w S c W c R W W r s C R p W C S r s C P
r p s r r p s p w s r w w s w c r c w c s c p c p
42. R C P R R R C C P W P W c S C R S P s p W S W S w
r c p r r r c c p w p w w s c r s p p s w s w s c
43. W w r C S R S W S C R P R R W C C S r W R W P P P
w c s c s r s w s c r p r r w c c s p w r w p p p
44. p s c S R S R w W C W r S s P c R W R P P C C w S
r c p s r s r s w c w p s w p w r w r p p c c c s
45. S p S P S W c w R P r P R S s W C c w R W S c p R
s c s p s w w c r p c p r s c w c r p r w s p w r
46. R P S c r w P W R r s R C W S S P W C P p s C C W
r p s p s r p w r w r r c w s s p w c p s c c c w
47. W R W c p S C S P r P w W C P C P R R r C S S W s
w r w r w s c s p s p r w c p c p r r c c s s w p
48. C P R C R P W w P C S S p P W p R R P s S C R S W
c p r c r p w c p c s s w p w s r r p w s c r s w
49. R W C r C R p C W W w C P S W P s R S P W S R S c
r w c p c r s c w w r c p s w p c r s p w s r s p
50. R W c R P W R p p C P S C c S s S C W C S P w C P
r w w r p w r r s c p s c r s w s c w c s p p c p
51. r c W p r P R c R W C C P pW w S P W w P C S S r
c s w r s p r r r w c c p w w c s p w r p c s s p
52. s S W C S C W C r C W w S R C W c W R R R P P R S
p s w c s c w c s c w p s r c w p w r r r p p r s
53. S S S R P S r C W R P P C W C R P W S c R C W P W
s s s r p s c c w r p p c w c r p w s r r c w p w
54. s P S r p S R c R s S W W C C w W R P P R R C W W
p p s s c s r s r p s w w c c c w r p p r r c w w
55. W R w S w P P C c r r s S P W C S S p S R R c P P
w r c S c p p c w w c r s p w c s s r s r r w p p
56. p C C W r W P S R S R P c C R S P R W R W s C S S
w c c w p w p s r s r p p c r s p r w r w c c s s
57. P w P r C W p R R S S C C P W W S C P S S W R c R
p r p c c w w r r s s c c p w w s c p s s w r p r
58. R P P W r P r S P W c R C W R w c s C S W s C P S
r p p w c p c s p w r r c w r s s w c s w r c p s
59. c s R R C W R w S W C S W R C r c w r c W p P w S
p w r r c w r s s w c s w r c p s r p p w c p c s
60. S c w W r p C R s W r c P w P r c w P w s r w r p
s s p w w c c r r w w p p c p c r s s s r w r p
61. P P P c R c R W C W S W s S C S w r W C W w P R p
p p p s r p r w c w s w c s c s r s w c w c p r r
62. s w W P S P p S P W p W R R C p R R S w p C P C C
p r w p s w p s s p w w w r r c c r r s s w c p c c
63. w W C W R S P S S R c W P P W P S p R p W C s P w
c w c w r s p s s r s w p p w p s c r r w c r p c
64. S W C C R r C c P p R R P w s W W P C R S W r c S
s w c c r s c p p c r r p r p w w p c r s w w s s
65. C p s w S C P w C R p S R W W s S W P R r P P S p
c w r s s c p p c r w s r w w r s w p r c p p s c
66. P R R w s R W W P r P C W pW P r C C C C s w R S
p r r s r r w w p p p c w s w p s c c c c w s r s
67. C R S p P C r W s C R W P c W P P S r W c S W R R
c r s c p c p w c c r w p r w p p s s w s s w r r

68.	S R R C C s r r c c	C P S W R c p s w r	P S s c s p s w p r	S C c P P s c w p p	W R rW C w r s w c
69.	C s c R W c r w r w	S P P r S s p p p s	S R rW W s r c w w	C C S P S c c s p s	R W P C w r w p c r
70.	S W R cW s w r p w	R W w S P r w r s p	P P W C C p p w c c	R c rC S r w c c s	P S S R p p s s r c
71.	R S W s s r s w w r	P P W R S p p w r s	S r rW W s p w w w	C c S P S c r s p s	P S S pC p s s r c
72.	wW S S P p w s s p	w c S w R s r s c r	P W C R R p w c r r	R C W W c r c w w w	S C P P C s c p p c
73.	S W P P C s w p p c	P S R S C p s r s c	R W w P p r w s p s	W C sR r w c w r p	C C R W R c c r w r
74.	P P S pW p p s w w	s C R P w w c r p p	R s R S C r r r s c	S R C S C s r c s c	C W W S P c w w s p

SYMPOSIUM ON *ESP* METHODS, SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY*

INTRODUCTION

J. B. RHINE, Chairman

LADIES AND GENTLEMEN: This is the third symposium on extra-sensory perception held in connection with psychological meetings in this country. The first was held in Madison, Wisconsin, last spring; and the second at Columbus, Ohio, last fall. This is the first of which the chairmanship and arrangement of the program were left to one who has reported evidence in favor of the hypothesis of extra-sensory perception. Whether this is evidence of evolution or merely Southern courtesy, I appreciate the invitation of the Society to take this part in today's program.

There has been of necessity a short time for the preparation of this symposium program. The papers have, I believe, all been prepared within the last week or ten days. There was not time to secure representation from certain points of view that might further have enriched the variety of the program. The arrangement that has been made is as follows:

We will, if there is no serious objection, incorporate Dr. Pratt's paper into the symposium program. The first part of the discussion will be on the mathematics of probability dealing with the problems of ESP. Two short papers will be given under this heading: the first by Dr. T. N. E. Greville of the Department of Mathematics, University of Michigan, entitled "Past Developments of the Mathematics of Probability with Special Relation to Problems of ESP." This will be followed by a paper by Dr. Joseph A. Greenwood of the Department of Mathematics, Duke University, on the topic "Some Unsolved Problems in the Mathematics of Probability Arising from the ESP Research."

* This symposium was a part of the program of the Thirty-fourth Annual Meeting of the Southern Society for Philosophy and Psychology, which met at Duke University and the University of North Carolina, April 7 and 8, 1939. The original papers are published in full here, with permission of the authors, and a stenotyped record of the discussion is included.

Next will come the experimental section, initiated by a paper entitled "A Further Advance in Methods of Investigating Extra-Sensory Perception," in which Dr. J. G. Pratt, of the Parapsychology Laboratory of Duke University, will review briefly the development of methods and give the results of his own experiments. Finally will come a paper by Mr. Charles E. Stuart of the Parapsychology Laboratory of Duke University, entitled "Some Unsolved Problems of Methodology." These papers will be followed by a general discussion.

In addition to these papers, it was desirable, I thought, to have a representation of the various points of view among the critics of the research in extra-sensory perception, and accordingly I wrote six of the most active of these critics located in the Southern half of the country, inviting their presence and co-operation in discussion, and asking them, in case of their inability to attend, to submit a brief paper to be read presenting their principal criticisms for consideration here. I have just now received a manuscript from one of these, Dr. Lemmon, of Washington University, and we shall hear this paper before opening the general discussion.

There is no attempt made in the arrangement of the program to marshal evidence on the subject of ESP. With the exception of Dr. Pratt's paper—which was a part of the regular program of the Society apart from the symposium—the question of evidence is not gone into.

A SUMMARY OF MATHEMATICAL ADVANCES BEARING ON ESP RESEARCH

T. N. E. GREVILLE

The current investigations of extra-sensory perception have suggested certain mathematical problems of great interest and a few of considerable difficulty which have been occupying the attention of a number of mathematicians in recent years. It is the purpose of this paper to summarize the progress which has been made in the solution of these problems. I shall discuss the subject under the following heads: first, a brief reference to the history of the subject; secondly, an enumeration of the results which have been obtained in connection with the more fundamental questions of the application of statistical and probability theory to data obtained in ESP investigations; and lastly, some mention of a few special problems which have arisen in particular phases of the work.

HISTORY

One of the alternative hypotheses which must be met in connection with any set of experiments purporting to demonstrate extra-sensory perception is obviously that of chance. Of all such possible hypotheses it is, in a sense, the most difficult to refute as it cannot be ruled out by any experimental precautions. It is possible only to compute the probability of a deviation from the normal situation as large as or larger than that actually obtained in a given case; and the decision as to whether the chance factor has been effectually excluded must be reached, in the final analysis, by a subjective process.

Nevertheless, the probabilities arrived at in connection with many of the reported ESP experiments have been of such magnitude as to raise an overwhelming presumption against the chance hypothesis in the mind of any reasonable person. It was natural, therefore, that the methods used in obtaining these figures should be subjected to close scrutiny. The book *Extra-sensory Perception* was closely followed by a series of criticisms of the mathematics used by Rhine and his collaborators, appearing, for the most part, in various psychological journals. Most of these unfortunately were ill-considered and wholly or partially without justification. Certain of them did, however, point out one feature in the calculations employed which was subject to criticism from a strict mathematical standpoint, although negligible in its practical effect. In all subsequent work the proper correction for this factor has been applied.

When certain of these criticisms began to be heralded in popular articles and even in some scientific circles as disproofs of the claims of extra-sensory perception, a number of mathematicians became aroused. They properly felt that, whatever might be the truth about ESP itself, the public should not be misled into believing that the experiments with ESP cards could be refuted by such unfounded assertions about the mathematical procedures used. As a result, much attention was given to the subject at the Indianapolis meeting of the Institute of Mathematical Statistics in December, 1937, culminating in the now well-known statement by Professor Burton H. Camp (2), then president of the Institute. This statement seems to have been generally accepted and at present the basic mathematics involved in this work is being challenged only by a small group of psychologists some of whom, so far as one may judge by their writings, are without extensive mathematical training. It is fair to say then that the period of heated controversy over the mathematical aspects of ESP research is at an end. Obviously, this does

not in any sense imply that the existence of extra-sensory perception has been proved, but merely that if fault is to be found with the reported work it must be, as Camp has said, "on other than mathematical grounds."

This controversial period had a great value in that it called attention to many interesting mathematical problems which might otherwise have escaped the notice of mathematicians at large. Much credit is due to Greenwood and Stuart for so clearly laying the groundwork for these studies in two articles in the JOURNAL OF PARAPSYCHOLOGY (8, 9). They suggested certain problems, which in turn suggested others, and now the problems which have arisen make up an imposing list. Some of these have been solved and some, as Dr. Greenwood will presently tell you, have not. These developments have formed the subject of at least ten technical mathematical papers, not to mention several now in preparation and perhaps others which have not come to my attention.

BASIC RESULTS

Coming now to the enumeration of the results obtained as regards basic principles, Greenwood and Stuart have shown (8) that the same conclusions would be drawn from all published experiments (except perhaps a few borderline cases) no matter which of several recognized mathematical techniques is applied to them. They have shown also (8) that the average or expected number of correct guesses (or "hits") with the standard ESP deck is 5, and that this is not affected by the particular pattern of the subject's call series. They have also drawn the important distinction between the "binomial" hypothesis in which complete independence of calls within the subject's call series is assumed, and the "matching" hypothesis in which it is assumed the subject will call each symbol exactly five times. Inasmuch as neither assumption is, as a rule, completely realized in actual experiments, Greenwood has further shown (4) that the variance of the number of "hits" is a function of the subject's call series; and that it has its minimum value of zero when the subject chooses to call the same symbol all the time, and reaches its maximum under the conditions of the matching hypothesis. That hypothesis, therefore, represents the most conservative assumption and the variance based on it may always be used with perfect safety. Generalizing this result, he has given (5) a formula for the variance of "hits" with an $m \times n$ target deck and any pattern of the call series.

Stevens (16) has shown that if the target deck does not contain equal numbers of the different symbols, the mean, as well as the

variance, depends on the subject's call pattern; and he has given formulas for both mean and variance under these general conditions.

Huntington (11) has indicated the usefulness of the Charlier series in obtaining the probabilities for short runs, where the unadjusted probability integral is not sufficiently accurate.

In a thorough analysis of a gigantic chance series, Greenwood (6) has verified empirically the basic characteristics of the chance distributions as previously demonstrated by mathematical analysis. This is most important as some writers had questioned whether the assumptions made in probability theory are realized with actual decks of cards. He found also that the variance associated with the call series of most subjects closely approximates that of the binomial.

SPECIAL PROBLEMS

Greenwood and Stuart (8) proposed the problem of computing the entire frequency distribution for the matching hypothesis. Huntington (10) computed the entire distribution for 3 x 3 and 4 x 4 decks and Sterne (14) obtained exceedingly close approximations to the 5 x 5 matching frequencies by calculating the first four moments and fitting to them a Pearson Type I curve. Subsequently, Professor Bancroft Brown (17) and I (3) independently arrived at the exact frequencies. My method is applicable to the general matching problem considered by Stevens.

Olds (13) has developed a most ingenious method for obtaining the moments, and theoretically the complete frequency distribution, for the matching of two arbitrary decks. However, for the latter purpose, his method is not of practical use.

Bubb (1) has given an accurate formula for combining "critical ratios" (i.e., the ratio obtained by dividing the deviation of the observed number of "hits" from the expected number by the standard deviation) which is preferable to the empirical formula suggested by Rhine (13, p. 34).

A more difficult problem, proposed by Greenwood (7), has been called the problem of "optional stopping." It is noted by ESP experimenters that the performance of a subject in routine ESP tests has shown a tendency to decline with use. For this reason, some experimenters would like to be permitted not to specify in advance the length of a set of experiments, but to terminate it optionally when experimental conditions seem to make this step advisable. Under these conditions, what probability should be associated with a given "critical ratio"? Alternatively, the problem may be stated thus: "What is the probability

that at some time in the course of a series of indefinite length, a given critical ratio will be attained?"

Greenwood and I (17) have shown independently that if there is no upper limit to the length of the experiment, the probability in question is unity. It then becomes of interest to know the value of the probability when the number of calls is limited to a very large but still finite number. This value is difficult to approximate and no one, to my knowledge, has succeeded in finding an upper bound which is sufficiently close to be of practical value, although Greenwood has given an empirical estimate on the basis of his study of a chance series of 500,000 calls. I have recently obtained a partial solution of the problem by the following scheme. The experimenter is required to select in advance one of the following three ranges:

Range I	1 to	100	runs of 25	(25-2,500 calls)
Range II	20 to	2,000	runs of 25	(500-50,000 calls)
Range III	400 to	40,000	runs of 25	(10,000 to 1,000,000 calls)

He must then make his experiment of a length lying within the range he has selected. It can then be shown (17) by suitable mathematical analysis that, no matter which range has been selected, a critical ratio of 5 will occur less than once in 50 times; a critical ratio of 6 will occur less than once in 200 times; a critical ratio of 7 less than once in 1,250 times; 8 less than once in 10,000 times; 9 less than once in 100,000 times; and 10 less than once in 1,000,000 times. These figures would indicate the adoption as a criterion of significance of a critical ratio of 5½ as roughly equivalent to the 2½ which is customary under ordinary circumstances. It should be emphasized, however, that the probabilities given above are merely upper bounds, not approximations. On the other hand, there appears to be some indication that the usual figure of 2½ may be too low (7, 12).

Greenwood has attacked this problem (17) from a different angle. Assuming the statistical distribution of ESP scores obtainable by chance to be approximately normal (an amply valid assumption for series of about 100 runs or longer), suppose that an experimenter obtains in succession several blocks of data, deciding the length of each block in advance of the execution of that particular block, and recalculating the critical ratio after each block on the basis of the total data up to that point. The experimenter is further required in advance of the entire experiment to agree on a maximum to the number of such blocks of data to be obtained, and then must not exceed the maximum so chosen. Finally, as the work progresses, the lengths of the successive blocks

must be determined so that the second block is not longer than the first, and no subsequent block is longer than the second. Under these conditions (which perhaps sound complicated, but really are not difficult to realize in practice), Greenwood has obtained a method of approximating closely the probability that any of the critical ratios calculated after the successive blocks will be as high as that actually obtained for the total data, and this probability would then be a suitable measure of the significance to be attached to an experiment carried out under the conditions stated.

After the mimeographed copies of my paper had been prepared, Dr. Greenwood pointed out to me the rather evident fact that under either my method or his it would be quite proper to take as the criterion of significance not the critical ratio for the entire data but the highest critical ratio obtained at any optional stopping place. While this may perhaps seem a bit "radical" at first, it should be observed that if the probability is one in a million of securing a certain critical ratio at any point in a series and such a value actually occurs, then something has happened which would happen by chance only once in a million times, and this calls for explanation just as much as in the case of any other type of event.

Some experimenters have observed a tendency for subjects to produce runs of scores both above and below the chance level, rather than high and low scores distributed in a purely random manner. Greenwood has recently devised (17) a measure of this tendency and has obtained formulas for the moments of its distribution, so that it will shortly be possible to translate an observed value of this statistic into a probability of its occurrence by means of tables which he is preparing for that purpose. I am indebted to Dr. Greenwood for the description of the two results last mentioned, and it is only fair to say that I have not had the opportunity to go over the mathematical developments in complete detail.

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SOME MATHEMATICAL PROBLEMS FOR FUTURE CONSIDERATION SUGGESTED BY ESP RESEARCH

J. A. GREENWOOD

You have just heard a discussion by Dr. Greville of some of the ESP problems of a mathematical-statistical nature that have arisen and been solved, at least partially. In particular it is interesting to observe that the matching problem, alone, has given rise to a considerable number of published papers.

It is my main purpose to give you a description of some of the problems which, as far as I know, are yet unsolved. The problems of this brief survey rather naturally fall into the three divisions: Selection, Miscellaneous, and Optional Stopping problems, and will be taken up in that order.

Many of the problems arise from an assumed characteristic of the ESP capacity as, for instance, when it is assumed that there is irregularity in its occurrence over short intervals. It is then desirable to be able to select out for separate evaluation any best consecutive runs preassigned or unpreassigned in length. I emphasize that I am not suggesting that grounds be invented for literally throwing away the other data. Perhaps I had better make a general statement of the problem covering all such cases. What is wanted is the antecedent probability that in the series one would be able to select out a predetermined configuration having a predetermined property at least as unusual in the

probability sense as that one actually selected or observed. That is of course nothing but a general statement of the application of probability to real conditions.

Of a more remote nature is the following discussion. Due to the very nature of most other statistical investigations the problem of considering anything other than the immediate results has not arisen seriously. But with the large number of official and unofficial researchers in the ESP field, and the relative rapidity with which large quantities of data can be accumulated, the problem of the various degrees of selection of data must be considered. The above problems involved the internal selection of data from a single experiment. On the larger scale the problem is how to select one or more experiments from a totality of similar experiments. As far as a single selection is concerned the familiar $1 - q^n$ suffices, and this has an obvious generalization for two or more selected ones with results on the safe side. I will illustrate it. Say j experiments out of n are observed to have probabilities less than or equal to p. Then from the binomial $(p + q)^n$ one finds the a priori probability that j or more of the n experiments would have a probability less than or equal to p as a result. But to show the inadequacy of the latter, two results with probabilities of .001 and .000001 selected from say ten experiments are accorded the same significance as a couple as if they had been .001 and .002, respectively.

This problem of larger selection and interpretation is not confined to ESP data alone. It enters, whether or not it is ignored, upon the second similar investigation that is undertaken. I think it is obvious that the probabilities obtained by whatever refined methods and conditions must finally be subjected to a common-sense interpretation. Thus I feel that the common-sense interpretation applied to an ESP experiment enters in the form of a reasonable assumption as to the number of similar, potentially reportable or publishable experiments undertaken. That is, as I see it, there are two probabilities to be considered by the experimenter: First, the a priori probability relevant to the occurrence of the phenomenon as observed in his one experiment; the other, the meaning of the result viewed as just one of many other attempts.

Naturally, for his own guidance, one will be more concerned with his own immediate results, and this may be a sufficient reason for completely ignoring the second suggestion. But for the crucial purpose of convincing others that he has something, he will on some occasion have to revert to the second consideration. I might add that there are several individual ESP experiments which will stand this test for long time to come.

In the second category a rather peculiar problem arises out of the ESP hypothesis. If it is assumed that the scoring level of two given subjects is about the same, then when the means of the two series are quite close, we should like some measure of their closeness in contradistinction to the usual standard deviation of the difference of two means. This measure would appear to be a function of the value of at least one of the deviation ratios obtained.

Of a radically different character is the problem of sequences of successes or hits within the single "run." Professor Uspensky gives, in his book on probability, a solution to the problem of sequences of successes in a fixed number of trials in the case of independent probabilities from trial to trial. There is here a clear case of a needed generalization to the matching cases, or at least a proof that the assumption of independence is sufficiently accurate in all the desired cases.

Pertaining to the ESP shuffle hypothesis arises the complex matching problem of the distribution of total simultaneous hits obtained by matching an ESP deck of cards with several other ordered decks. This can be considered either from the viewpoint of unknown random order or from known order of the target decks. And from the same hypothesis we get what may be a trivial problem of, say, finding the probability that a randomly shuffled deck will result in three or more circles being adjacent. Several types of correlation problems have arisen recently. I shall not give them room for statement in this paper. However, an offshoot of one, the solution of which is unknown to me and the lack of which prevents a generalization of another is the following: If x and y are identical correlated variables, what is the correlation between x^n and y^n?

The third type, or "Optional Stopping" problem described by Dr. Greville has, it seems to me, more ramifications and certainly more difficulties ahead of it than the problems already considered. The partial solutions given by each of us, while loosening considerably the straitjacket placed on the experimental procedure (i.e., effectively naming in advance the number of runs to be undertaken in the experiment), falls short of ideal conditions. Naturally, complete freedom in ceasing the experiment is desirable provided not too much significance is lost in the process.

Even more freedom is suggested by an optional choice of an a posteriori stopping point for evaluative purposes. This is seen to be somewhat of a selection problem already stated in which one endpoint of the selected sequence is fixed at the beginning of the series. These procedures, while ignoring the actual remaining *values* of the series,

must take into account the *number* of such values. As was indicated by Dr. Greville, approximations to these are already solved.

It is hoped that this paper has given you an idea of the type of problems of a mathematical-statistical nature that we encounter in this field of research. Perhaps some of you will find these as stimulating as several of us have in the past.

A FURTHER ADVANCE IN METHODS OF TESTING EXTRA-SENSORY PERCEPTION

J. G. PRATT

This paper will describe a new procedure for the testing of clairvoyance. As an introduction it is necessary to survey briefly the development of experimental methods in this field to the present time.

When we use the term "extra-sensory perception," we naturally think of the investigations which had their beginning at Duke University about 10 years ago. However, organized work in the field generally known as psychical research dates from about 1880. In so far as that dealt with the problem of the occurrence of ESP, a wide range of testing materials was tried throughout the years. Many tests were made with numbers, playing cards, and other restricted items which gave results subject to a strictly quantitative evaluation. At the same time, many experiments based upon an indefinite class of items gave results of a qualitative sort: i.e., results which were not to be judged directly by mathematical criteria of significance.

Early in the Duke investigations, the simple material now so widely used was adopted as the basis of these tests: five symbols upon cards which were usually used in packs of twenty-five, five of each symbol. Card material offered the most convenient means of obtaining a random series of events, essential to these experiments, and was at the same time readily adaptable to testing different aspects of the ESP hypothesis.

In tracing the development of experimental methods, it will be convenient to treat separately the advances affecting the relation between the subject and the card to be perceived on the one hand and, on the other, those concerned with recording and reporting the data.

RELATION BETWEEN THE SUBJECT AND THE EVENT TO BE PERCEIVED

This discussion is confined entirely to methods for investigating clairvoyance, in which the subject was usually tested for his knowledge of symbols upon the concealed faces of shuffled cards. At the outset of the Duke work, the pack of heavy, hand-stamped cards was

placed face down before the subject. He called the top card, then removed it to one side without looking at the face, called the next before removing it, and so on through the deck. This was the "before touching" or BT condition. A natural development from this was to have the subject call down through the shuffled pack without removing any cards, the DT condition. Corresponding to the BT procedure, open matching (OM) was introduced to permit a motor form of response making use of the card sorting principle. In this test, of course, the cards were sorted face down, usually at the subject's own preferred speed. These tests were all used in an exploratory stage of the investigation. The results obtained from them were valuable chiefly in offering encouragement for further testing under more rigorous conditions.

The first advances were in the direction of introducing distance between the subject and the cards. Separations of one and two rooms, hundreds of yards, and even many miles were introduced without lowering the level of scoring of the successful percipients.

The expediency of working in the same room with the subject led naturally to the use of opaque screens between the percipient and the cards. In early tests under this condition the subject handled his own cards under a screen which completely blocked his view of the process. Later, the cards were handled entirely by the experimenter, who held them out of sight behind a convenient screen. In the same class, experimentally, as the tests with screens are those made with cards sealed in opaque envelopes. All these later conditions, providing complete safeguards against the effect of sensory cues on the scores, have given results which far exceed chance expectation.

DEVELOPMENTS IN METHODS OF RECORDING

At the beginning of the ESP investigations, the method of checking the score was that which generally prevails in other fields of scientific research; the investigator made his observations, checking the order of the pack of cards against the subject's calls, with the latter person as an additional observer. The position of the correct calls in each pack was indicated on the record and the score for each run of twenty-five cards entered. More than ordinary care was taken to preserve all records intact and to report all without selection.

The first advance to be made was the introduction of a third person as a witness to the testing and checking. This offered some additional safeguards against the occurrence of incidental errors in recording.

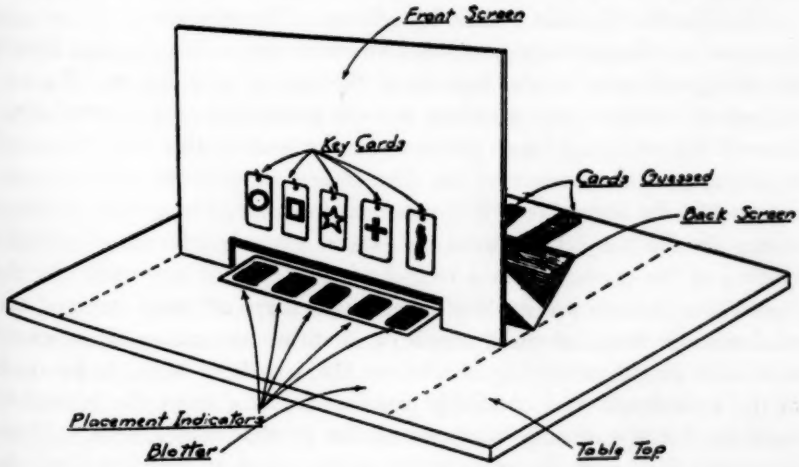
An important advance was introduced naturally in connection with some of the experiments involving distance. This consisted of having the records of the subject's calls and of the cards made entirely independently. These records were made by different people (subject and experimenter) on separate record sheets with no possible influence of one record upon the other.

Some progress has been made in the direction of entirely removing the human element from the recording process by the use of mechanical devices. Tyrrell in England has made considerable use of a machine which automatically selects an event to be perceived and records the subject's response. Several similar devices are in an experimental stage of development in this country and it is possible that something in this direction will constitute the final advance in recording safeguards.

THE FURTHER ADVANCE IN METHODS REPRESENTED BY THE INVESTIGATION HERE REPORTED

The experiment to be described is one now in progress in the Parapsychology Laboratory as a joint project of Mr. J. L. Woodruff and myself. The work is a general investigation into the effect of size of symbols upon ESP scoring. However, this aspect of the work is not yet completed. The general method of testing ESP will alone concern us here. The plan of the remainder of this paper will be, first, to describe the procedure in general terms; then to give the general results; and, finally, to demonstrate concretely the actual procedure.

Safeguards Against Sensory Cues. The experimental procedure used was a variation of that known as "screened touch matching" or



STM. A vertical screen (represented here by figure), two feet wide and 18 inches high is placed between the experimenter who deals the cards and the subject. This has a small aperture at the bottom allowing the experimenter to see the subject's responses but blocking the subject's vision of anything on the experimenter's side. An advance over previous uses of this condition consists in having the key cards by which the subject makes his indications hanging on the surface of the screen on little pegs out of sight of the experimenter handling the cards and in an order unknown to him. The key cards are rearranged just before the beginning of each run either by the subject or by the second experimenter, and the subject's responses are made by pointing to positions corresponding to the key cards in the opening under the screen. The experimenter behind the screen (Woodruff) thus deals off the cards, following the subject's pointer, into five piles without any knowledge of how the subject is identifying each card. This feature avoids all possibility of such volitional errors on the part of the experimenter as might cause him to vary from the subject's responses in a way to increase the number of hits.

The cards to be guessed are shuffled five times by the second experimenter (Pratt) and are given a final cut with a paper knife after the screen is placed in position for the run. The subject makes his responses at the speed he prefers. There are great individual differences but the average rate is around fifteen seconds for the run of twenty-five cards. The conditions, as will be clearly seen from the demonstration, guard against the use of cues on the part of either the subject or the experimenters.

Safeguards Against Recording Errors. These consist of features designed to eliminate any possibility of errors in checking arising from a knowledge of what would have been the correct distribution of cards, as well as features safeguarding against accidental or volitional selection of the records in any manner. At the end of the run, Woodruff makes a complete record of the distribution of cards in the five positions while the screen is still in place. Meanwhile, Pratt, who has been sitting on the subject's side of the screen out of sight of Woodruff's dealing of the cards, makes a record of the order of key cards for the run. These records are made on sheets which are officially stamped and marked with identical serial numbers. A different number is associated with each run by assigning in advance the length of series to be made in the experiment and obtaining numbered sheets from the laboratory recorder for the specific purpose of the predetermined series. These sheets are filed with the recorder when the series is completed and the

unbroken sequence of numbers is a completely reassuring safeguard on the question of selection of data.

In addition to the record of the order of key cards, Pratt's record shows on each sheet the name of the subject, the date, the type of test, and the initials of the two investigators. When each experimenter has completed his independent record, the two sheets with identical numbers are clipped together and deposited in a locked box to be compared later and scored by a third person.

The two experimenters then jointly check the run score by comparing the distribution of the twenty-five cards with the order of key cards and sorting out and counting the hits. Each experimenter enters the score from this immediate count in a personal record book.

When the record sheets themselves have been checked, a comparison is made between the scores found from them and those kept by the experimenters at the time of the test. In case a discrepancy is found, the written records are consulted at once to see whether an outright oversight occurred in checking. If a re-count shows that such was the case, the score on the record sheet is revised. If the re-count does not show a checking error, the lower score is always taken as the official one. If, for example, the experimenters have a score of 6 for a particular run and the record sheets for that run give only 5, the investigators change their score to 5 and use that figure in all calculations from the data. Actually, in 2,400 runs, only three such discrepancies, each of one hit, were found. The effect of these errors, if undetected, would have been to raise the score for the entire series by 3 hits.

In every instance it has been evident that the error was made by one of the experimenters in making his independent record. The errors in recording were such as could be detected by the presence of six of one symbol and four of another in the card distribution, or by having one symbol appear twice in the record of the key cards. These facts show that, as far as our experiment goes, counting the score from the cards at the time of the experiment is more accurate than getting it from independent records. However, even in the face of the evident errors in the written record, the lower score was taken as the "penalty" for the discrepancy in all three cases.

Results. With the procedure described, thirty-two subjects, chiefly undergraduate women students of Duke University, were tested between the first of October, 1938, and the last of February, 1939. During this time, they made 2,400 runs or 60,000 individual trials. The hits numbered 12,489 or 489 in excess of mean chance expectation. If we evaluate this deviation by means of the formula for simple sampling,

S.D. $= \sqrt{npq}$, we find that the S.D. is 98 and the deviation ratio, 5. However, as 2,400 runs was not set at the outset as the limit of the experiment, it is necessary to revise this deviation ratio to make it comparable to the scale for simple sampling on which a value of 2.5 or 3.0 is considered significant.

Actually, the experiment as reported is made up of six sub-series, the length of each of which was fixed at the start. The problem of allowing for the effect of optional stopping after an indefinite number of such fixed sub-series is a new one for probability mathematicians. Dr. Greenwood of Duke University, on the basis of an unpublished solution of the problem, kindly calculated the probability for our series as corrected for optional stopping and found the value to be 5×10^{-6} instead of the uncorrected probability, 3×10^{-7}. In other words, the uncorrected deviation ratio of 5.0 is equivalent to a deviation ratio for simple sampling of 4.4. Thus, the results are clearly beyond what might reasonably be expected by chance occurrence. In view of the conditions under which these results were obtained, we are compelled to regard them as "inexplicable" on any hypothesis unless it be that of an extra-sensory mode of perception.

Demonstration.*

SOME UNSOLVED PROBLEMS OF METHODOLOGY**

C. E. STUART

The problems of extra-sensory perception arise from the daily-life events generally classified as "psychic experiences" which can better be designated as *parapsychic* until their relation to the recognizedly psychic is cleared up. Examples of them are too well known to warrant discussion here. Most of them concern acquisitions of knowledge the means of which the subject cannot fit into his conception of the more or less orderly nature of his world. The psychologist, when consulted for an explanation, has the task of fitting the experience into his more extensive knowledge of the orderly working of human nature. When his explanation is questioned, the only resort is to experiment.

Since the experimentation is to discover whether there is some perceptual function generally common to the parapsychic experiences and not adequately deducible from contemporary systematic psychologies,

* In the demonstration Mr. Burke M. Smith acted as subject, with Mr. J. L. Woodruff and Dr. Pratt as experimenters. The scores for the two runs were 5 and 11 hits per 25.

** Read by Dr. Bernard F. Riess.

the first problems of experimental method naturally follow the directions outlined by Dr. Pratt in his paper. That is, can we, through laboratory experimentation, find some measurable performance which does not permit of reasonable alternative explanation and which *might be* a common factor in parapsychic experiences?

Granting the success of this endeavor experimentation might then proceed in either of two directions. First, we may accept what has been found in the laboratory test as operationally defined by the methods used, and try to find the further orderly relations concerning that principle. This is in effect what has occurred. ESP is what experimentation in extra-sensory perception under certain crucial conditions has produced. What is the effect of distance on ESP? Of rate of performance? Of drugs? Etc. These findings become entirely empirical additions to our operational definition.

The second direction of research might be to change the essential features of the ESP test to approximate more and more the daily-life situations from which the problem originated. The question would be whether, after all, this laboratory finding is related to the every-day parapsychic experience. In this case, our operational definition of ESP would be such common factors as could be pointed out in veridical parapsychic experiences. The results of laboratory tests would then be viewed in the light of their being more or less close approximations to these "pure cases."

An example may make this thesis clearer. One of the outstanding features of the parapsychic experience is its generally sporadic nature. It just happens, apparently without much relation to the activity of the subject. The subject of an ESP test, however, reports at a certain time for a specified period of work. This latter condition is comparable to asking subject Jones to come up to the laboratory at 2:30 Wednesday afternoon and have a veridical parapsychic experience while two observers watch him and check up.

Now if the sporadic, inconstant nature of the parapsychic experience is a real quality of the process of extra-sensory perception, then the laboratory appointment is methodologically bad, and some technique should be devised to allow for that quality in the working principles of the experiment.

The present discussion is concerned then with noting both discrepancies and similarities between the ESP test and the every-day parapsychic experience with a view to seeing what methodological advances in the laboratory tests are indicated.

1. The parapsychic experience is sporadic, inconstant, and generally unreliable as a guide to action. This is true even of trance mediums and professional clairvoyants. Although the difficulty of objective tests of professional clairvoyants is notorious, even when their performances are given every possible credence, many of their assertions are simply untrue. In general, however, we may note that some individuals appear to have more such experiences than others, and some seem to have more reliable experiences than others.

An ESP test, then, should show wide differences in individual performance. And they have done so. The methodological question is that of finding an objective technique for selecting individuals of the greatest reliability. The tests have also shown that individuals are inconsistent from day to day, and that even reasonably consistent high scores decline or disappear with time and monotony. Techniques for testing the predictability and discovering the causes of such scoring changes and weighting the results accordingly are needed. These techniques might involve experimental criteria (such as performance in some non-test task) or delicate mathematical methods for analysis of scoring changes.

2. The parapsychic experience usually deals with events of personal emotional significance to the subject. The ESP test, with its simple guessings of uncomplex symbols, varies widely from this condition. And one could question why the test situation works at all. But frequently events of emotional significance are objectively trivial. What has been noticed is the importance of the influence of the experimenter and the social situation upon the subject's performance. Differences in the nature of the stimulus objects as such seem to make no reliable difference in the scoring.

What is suggested is that the subject should be personally interested in the outcome of the tests and should feel that the experimenter is himself interested similarly. The problem here is the objectification of these vague social skills into some interest-giving element in the situation itself; that is, to find the psychological essentials for such mutual interest and to objectify them in the experimental methods themselves.

The importance of the emotional background of the laboratory situation is easily deducible from wholly psychological considerations. If we forget the problem of ESP for a moment, Dr. Pratt's demonstration could be described as simply the presentation to a subject of an impossible task. Dembo used an essentially similar technique in Lewin's laboratory for the study of the genesis of anger. The only difference between the ESP test and a pure frustration situation lies, therefore, in the role played by the experimenter.

The question of using material of directly emotional significance to the subject remains, however, largely unexplored, so far as quantitative estimation with mere symbol tests are concerned. One utilization of this factor probably occurs in the attempted reproduction of free drawings, wherein the emotional significance of the stimulus object can at least be postulated. Conditions for control of alternative hypotheses and for accurate statistical evaluation have recently become available, and wider use of the method is to be expected. What is still needed is a valid method of psychological analysis. The interpretation of drawings, much as the latent content of a dream is inferred from its manifest content, might seem to match the conditions of the parapsychic experience more closely than any other evaluation. Further consideration of the generally emotional aspect of the test situation might involve adaptation of word and free association techniques.

Specific emotional factors can and have been introduced such as rewards, competition, "play," and the calling upon the pride of the subject or his friendship for the experimenter. These latter, however, vary in effect with the social skill of the experimenter.

3. Another feature of the parapsychic experience is its occurrence frequently during periods of general abstraction. The ESP test situation may vary widely from this requirement, but it has been generally noted that distractions decrease scoring. While the experimenter may strive to keep the subject from feeling self-conscious, he must frequently fail. The need suggested is for techniques which allow the subject to work alone, and at his (rather than the experimenter's) convenience. This need may probably best be met in one way by wholly mechanizing the test procedure. Several ESP machines have been devised, and the requirements such machines must meet have been recently summarized by Dr. Rhine. The machine should be technically capable of excluding alternative hypotheses adequately, should be completely reliable in performance, and should be amenable to operation with little attention or physical effort on the part of the subject. But the use of this method is as yet untried and many others may be needed before the problem is adequately solved.

Many of the foregoing suggestions might well be incorporated in solution of a problem which must ultimately concern the ESP investigation—a universally repeatable experiment certain to yield positive results. There is no a priori reason to expect that such an experiment may be set up with the methods now available. And even the formulation of requirements would scarcely be agreed upon today. But we do know that the repeatable experiment must meet rigorous requirements for the

exclusion of all but the ESP hypothesis yet lay as few restrictions as possible upon the hypothetical possibilities of the nature of the ESP process itself.

THE ROLE OF SELECTION IN ESP DATA*

VERNON W. LEMMON

Criticisms of ESP experiments by psychologists have fallen into three major categories: a) mathematical evaluations of departures from chance expectations; b) the possible effects of sensory cues; 3) the effects of various kinds of non-random selection of data.

Since mathematicians have recently shown considerable interest in the first category, it is perhaps time for psychologists gracefully to retire, and leave this field to those more competent to handle it. The question of sensory cues has been discussed at great length, and researchers in ESP have shown a willingness to modify their techniques in order to avoid such effects.

The possible effects of selection were discussed by the present writer in a previous article,[1] and the question has been persistently revived by many other critics. At the same time the ESP researchers have consistently denied that these criticisms have any validity. It is quite possible, therefore, that the critics and the criticized have not fully understood each other, and that the matter deserves further clarification.

Clarence Leuba, in a recent article,[2] states that non-chance results can be obtained from chance data if a) "lucky" subjects are used, and if b) runs are stopped at favorable points. These two conditions are probably not entirely independent. If chance alone is operating, there can be no truly "lucky" subjects. To be sure, a subject may have a long run above chance, but if he continues long enough, there will certainly be compensating runs below chance. A subject can only be made to appear lucky by arbitrarily stopping his run while he is still scoring above chance. Thus the first part of Leuba's criticism seems to be included in the second.

The ESP researchers have steadfastly maintained that a true chance series could be stopped at any point, and resumed at any point, either with the same or a different subject, without changing its chance character, since the earlier runs have no influence whatever upon the later.

* Read by Mr. Burke M. Smith, Duke University.

[1] LEMMON, VERNON W. Extra-sensory perception. *J. Psychol.*, 1937, **4**, 227-238.

[2] LEUBA, CLARENCE. Has recent research undermined the evidence for extra-sensory perception? *J. Appl. Psychol.*, Vol. XXII, **6**, Dec., 1938, 549-553.

These contentions are of course true, but they do not cover fully all aspects of the question.

Let us assume that a subject is making pure chance guesses, and that a *cumulative* average of his runs is being kept. This average will fluctuate both above and below chance expectation. Let the run be terminated while the cumulative average is above chance, and the value entered in the data book. Now let a new run be started, with either the same or a new subject. These guesses will be totally independent of the previous guesses; the cumulative average may at first trend high, low, or normal. However, if the series is only continued long enough, there will certainly come a time when the cumulative average will experience a fluctuation above chance expectation. Let the run be terminated at this point, and the value of the average entered in the data book. It is easy to see that if this procedure should be exactly repeated a hundred, or a thousand, times, the data book will still contain nothing but "plus" values. At the same time the averages, taken as a whole, will include every guess that has been made, and it can truthfully be claimed that not a single case has been neglected.

It has repeatedly been claimed that if every case is included in the results, high and low averages will tend to occur in equal numbers (if chance alone is operating). Yet the discussion above shows that this is not necessarily true. Where is the paradox?

It seems to the writer that the data book averages just described really constitute a series of "readings" or "observations" on a fluctuating quantity, and that, furthermore, they constitute a selected series, even though every case has been included. It would have been equally possible, with the same subjects, to have selected a series of observations every one of which would be below chance expectation, or exactly at chance expectation, and in these cases also, every guess would have been included.

Selection is thus not avoided by merely including every guess which has been made. It is necessary to go a step further, and make sure that the "observation points" (i.e., the stopping points of the runs) are not selected either. This can be done by stopping the runs according to a criterion which is totally unrelated, directly or indirectly, to the state of the score. Practically, the easiest way to accomplish this would seem to be to determine in advance of each test just how many guesses are to be included, as has been suggested by practically every critic writing on this topic. Even this method is not entirely free from possible criticism. For example, the experimenter might decide that since the subject was doing very well today he would set a limit of twice as

many runs as usual. Here selection has crept in again. Perhaps a machine which would ring a bell after a variable interval, unknown to the experimenter, would be the only really satisfactory solution.

DISCUSSION

The Chairman:

Because Dr. Lemmon's criticism raises mathematical questions, I will ask Dr. Greenwood or Dr. Greville to open the discussion.

Dr. Greville:

I think there is a fallacy in the argument presented in Dr. Lemmon's paper. It is a rather subtle one, but I think it is possible to make it clear. Suppose for the sake of argument that we are going to make a certain number of guesses; at least we are going to terminate this series of guesses at a point where the average score is above the chance level. We are going to get a certain average score and then we are going to start either immediately or at some later time another set of guesses, keeping this up until we reach a point where the average is again above chance level. We are going to put that down, as he says, in the data book in a similar way. We are thus going to get a lot of other score averages all of which will be above chance. We will put all of those down. When we get through, we will average all of them up and get a result which will certainly be above chance. Now that would certainly follow, if the unbroken series of averages above chance is given. Let us examine this assumption. If nothing more than chance is operating we might properly stop for a while for any reason and then go on again. We might change to another subject; (that is the same thing as if we used a different pair of dice, if we were throwing dice; or if we were in a bridge game and someone got up and walked around the table—some people think that will change your luck). But for whatever reason we stop, suppose we think of all of these trials as one long series that we wanted to divide up into parts; and suppose we are free to choose our points of division. Now let us say the average of this whole series is slightly above chance, but if we compute the average at various points, as long as it fluctuates sometimes above chance, of course we can pick out places where it is above chance. In that way we would have a series of blocks of scores in the data book that would add up to something above chance. On the other hand, we could just as well divide the total series at points that would average below chance, picking out the bottom of these troughs (referring to diagram). (Of course the last one would have to be above.) In this case all of our averages in the data book, except the last one, would be below chance—only the last figure being above—yet we get the same total as we would by the other procedure. I don't think that criticism is really mathematically sound.

Dr. Rhine:

Have you anything to add, Dr. Greenwood?

Dr. Greenwood:

Nothing except possibly to make the argument more devastating. And that is because it is not certain that in a finite series such characteristic division points as Lemmon requires could be found. But granting that fact, all that Dr. Greville has just said would still be good.

Dr. Rhine:

One thing you would grant, I suppose, is that at the end of the series one can stop, if he is allowed optional stopping, with a little deviation above or a little below, as may be desired. This point you have already discussed and some of the advances in mathematical methods you have presented today would be applicable as a slight correction.

The meeting is now open for general discussion. If you will be good enough to give your name and institution for the record when you speak, I shall appreciate it.

Dr. Riess (Hunter College):

I am not clear on Dr. Pratt's new method as to how he is taking care of the criticisms we have been hearing concerning possible auditory cues from the shuffling of cards. How does he guard against the auditory cues from the adhesion of cards of different symbols? That was a slight criticism made at Columbus—that one of the hypotheses for high scores in ESP is the possibility of such cues in card shuffling and card drawing. In the method that Dr. Pratt is using, it seems to me the possibility is there for that type of objection—not that I think the criticism iself is worth very much—but it has been made as one which we do have to answer.

Dr. Pratt (Duke University):

Mr. Chairman, if Dr. Riess could make more explicit the principle supposedly involved, I think we might see more clearly just what this criticism might be worth. As far as auditory cues during the shuffling of the cards which the subject is guessing are concerned, that is done by the second experimenter and is not of importance for determining the order in which the cards are at the time the run starts, since, as we know, they are cut with a paper knife behind the screen as the last thing before the experiment begins. How there could be any indication from the sound of a paper knife in cutting the pack as to what the card on the top or the bottom of the pack is, I don't know; but perhaps it is possible to conceive that the paper knife vibrates differently for each symbol. At some point in these criticisms judgment has to come in.

Now, it is perfectly possible to devise an experiment about which no one would ask this question at all, because it would be obvious that the distance was such as to make it absolutely ridiculous. We have used the present procedure for the advantages, we might say, which it offers the subject and for the difficulties or the attenuation which it gives to the criticisms. In our procedure it is easy to see that a very complicated interchange of cues would be necessary if the results were to be affected at all. We would have to imagine, if any assistance is to be obtained from cues, that the subject

first gives the experimenter some hint of the order of key cards. The experimenter would have to use the information so gained in misplacing, consciously or unconsciously, the cards from the packs or in indicating by cues to the subject what the next card really is, the subject then making his next indication so as to get a hit. All this would have to occur in something like half a second. It becomes for this procedure simply a question of judgment as to whether such cues could occur and affect the results.

Dr. Riess:

I don't want to be a Devil's advocate but these possibilities come up all of the time in criticism. I am not sure whether Woodruff was able to see you as you sat in the room back of the subject but that is a criticism that is bound to come up when people who are more than unusually skeptical of the experimenter's honesty are involved and there are criticisms of that kind.

Dr. Pratt:

Yes. . . .

Dr. Riess:

I would like to put in a word of warning against the use of any method which is open to criticism of that type.

Dr. Pratt:

Well, the word is well received as far as I am concerned. At present our experiment is in a stage in which perhaps no such criticism would be raised, but we are just starting. The results haven't yet reached the point that we may consider them, I will say.

Dr. Miner (University of Kentucky):

May I simply make a suggestion as to what I think would be interesting to those who are studying ESP. I assume in some series or trials or runs you occasionally have a failure. Is that possible?

Dr. Rhine:

Yes.

Dr. Miner:

My suggestion is that, when you do have a failure, you publish a list of the explanations of that failure which you might give—just a list of the explanations which you regard as sound for that failure.

Dr. Rhine:

May I ask one question for further elucidation? Do you mean the explanations which we feel certain are experimentally established or such hunches, as we might have?

Dr. Miner:

Your various hunches as to the explanation for that failure, just a list of what you might regard as explanation for that failure.

Dr. Rhine:

That might lead to the charge that such explanations are "alibis." Has anyone any comment to make on that?

Dr. Greville:

I suppose this might be done in order that other people trying to repeat the experiment can avoid those same causes of failure?

Dr. Rhine:

Would that not be a part of the need for a general discussion of all that we know, suspect, and guess as to what contributes to either failures and successes?

Dr. Miner:

Your suggested explanation of failures and successes. I think it is desirable that we have some list of interpretations which might be used in judging ESP.

Dr. Rhine:

I might say that we contemplated a survey of research articles beginning back with the impressions of those that worked with the subjects long before we began at Duke, adding on such tentative judgments as more recent work has led us to have. I think we have been a little shy about presenting this for the reason that it exposes us a good deal to the suggestion that we are trying to apologize for failures or trying to read in unestablished conclusions. But I think the project you mention is a very important one.

Dr. Miner:

I don't object to delay until you have a critical series of failures as well as a critical series of successes.

Mr. Crissy (University of North Carolina):

There is one point I would like to raise. I don't know enough about your experimental set-up to be sure whether it is a contribution or not. I am interested in tests and measurements; hence I can't help thinking of the present problems in terms of, say, a testing procedure. Let us say that someone shuffles and records the pattern in ten decks of cards and that the record of this pattern is duly signed and sealed. A week later in the laboratory an experimenter uses these set cards as the run cards and has a subject call these cards by one of the acceptable techniques. After collecting the data (records of the 250 calls) an inter-item matrix could be easily computed (250 x 250) and analyzed for internal consistency by the Kuder-Richardson procedure (Kuder, G. F. and Richardson, M. W.: The Theory of the Estimation of Test Reliability: *Psychometrika:* vol. 2, No. 3, September 1937). This treatment, I believe, would place emphasis upon the individual call as the elements of ESP (rather than the run as was reported by the gentleman from Columbia who used odd-even runs). If you used such a procedure on the data from Dr. Pratt's experiment I believe you would get an estimate of reliability of a test which you would designate

probably a test of ESP. I don't know whether this would be contrary to the usual experimental set-up. I must confess, however, that when I read some of the articles I kept wondering to myself: How reliable is the test?

Dr. Miner:

In view of the large amount of work you are doing, there are some things which are of very definite advantage that would come out of it, if you are willing to devote the time. I think the main difficulty of psychologists is wondering whether they should devote that much time. Two things, it seems to me, might come out of the research: First, the stimulation to mathematical analysis and secondly some idea as to the nature of the patterns of guesses if these were treated really as patterns of guesses.

Dr. Rhine:

Is there any comment from anyone working in the field? It seems to me there might be something said first, as to the question raised by Dr. Crissy. How close are you coming to meeting this question in your A and B series, Dr. Pratt?

Dr. Pratt:

I am afraid it doesn't come very close.

Dr. Rhine:

Perhaps you will explain what your procedure is.

Dr. Pratt:

For a period in our work we divided our subjects on the basis of performance up to that time into A and B classifications, A being those that we thought would do better in the remainder of the tests, B, those that we suspected would do poorer. For a time we found a slight difference in favor of the A's in subsequent performance, but not a significant difference between the two groups. Our subjects, in other words, were selected without reference to ESP ability, and were largely unselected throughout the period of working. It seems that, whatever deviations we obtained and however they are to be explained, there was no reliable basis of prediction on past results as to how the subjects' scores would be divided in the future. However, an acceptable measure of reliability would depend upon fractionating the data within the same day, I judge, or in such a manner that you could be sure the conditions were more nearly comparable. It wouldn't be desirable to try to determine the validity of the test by taking the performance of a subject for one month and comparing it with the performance for the next month. We could make some such division within the day, taking even and odd runs for the same subject, and arrive at a conclusion as to whether there is any significant correlation.

Dr. Riess:

I have been trying to get at some measure of reliability and the correlation for one subject who got a significant score in my series on odd versus even trials for the calls alone, uncorrected, was .71. Of course that is an

exceptional case. I am going to do the same kind of split-half technique for the subjects who score chance.

Mr. Crissy:

One of the things of course I had in mind there was this interesting sidelight, a computation à la Kuder-Richardson could then be compared with reliability computed for the same data but by some fractionation of runs, say odds against evens. Also you would find it appropriate probably to apply both techniques to "good" runs (demonstrative of ESP) and to "bad" runs (chance figures) and see whether the "test" measures ESP as consistently as it does the lack of ESP.

Dr. Miner:

One of the suggestions that came out today should be published pretty broadly, and that is the suggestion that the phenomenon, so far as you can judge, is sporadic and therefore up to the present time you can see no use for it except from a pure science point of view. It seems to me that you might caution the public from trying to put ESP to use before it were justified.

Dr. Rhine:

We waste a lot of postage and paper trying to discourage some enthusiasts on that point. A general warning should be very helpful.

Dr. Shea (U. S. Forestry Service):

Dr. Rhine, have you ever noted in your experiments degrees of what we call success and failure of people when they are ill for any reason? I think that might throw some light on this problem. That is, take any of the minor illnesses—headache, menstrual period, or anything else—and observe deviations that occur in the general trend of successes or failures during periods of so-called illnesses.

Dr. Rhine:

There have been a number of observations made on the health of the subject, incidentally in the course of a long series of tests. There have never, of course, been any experimental illnesses induced. One is likely to run into the objection that the experimenter, knowing what the score is and observing the illness, unless it is carefully observed and recorded before any tests are made on a given day, may give an interpretation that is not entirely objective. In the earlier investigations which were not done under conditions which we now impose on ourselves but which we feel are very much worth considering in view of the results from the later work done under better conditions, there were several subjects ill at one time or another. The most common illnesses were colds and influenza, and there was notable falling off with such indispositions. That was published for what it was worth, not as a conclusion of the final order that we would like eventually to reach.

The other point I would like to mention is the case of two subjects who have been studied at considerable length and reported to have lost their

ability to perform following medical treatment. In these instances the illness was apparently a favorable condition. That is naturally a generalization which we would not want to make for other cases. In the work reported by Dr. Riess he got remarkable results under excellent conditions until his subject was taken to a physician and treated. (I believe the diagnosis was hyperthyroidism). Results of tests made following treatment approximated chance. They had been exceedingly high before. There is another case involving thyroid disorder which Dr. Drake studied. His investigations showed apparent ESP but he reports in a later unpublished work that his subject, a boy, having undergone treatment for hypothyroidism is now unable to do the formerly good scoring he had done.

Dr. Greenwood:

Practically all of the experiments have been carried out with the view to getting a significant deviation ratio on the mean. I wish to suggest that there may be other measures of ESP performance, perhaps unknown at present, due to some unknown characteristics of ESP itself. In fact, there might be a dozen entirely different statistical measures of such a faculty. For example, the variance would measure an unusual dispersion of the scores. New hypotheses relevant to ESP can call for new measures of it and vice versa. Theoretical considerations and generalizations with no more apparent application than a game of chess have frequently pointed the way to correct hypotheses.

Dr. Rhine:

That has a good echo in my mind from the views of a group of psychologists at another university. I don't know that I should speak of their work before publication; but they report a very subtle effect that is not dependent for evaluation upon the methods we ordinarily use, the deviation from the mean. One of these experimenters is present. I wonder if you would want to say anything about your findings, Mr. Taves?

For Mr. Taves' approach the reader is referred to the article on p. 38 ff. of this issue.

Dr. Rhine:

If it isn't clear, that article is coming out in the JOURNAL OF PARAPSYCHOLOGY in the next number. It can be read in full detail. I take it this is such a case as you had in mind a moment ago, Dr. Greenwood.

Dr. Greenwood:

Yes.

Dr. Rhine:

I think we need to keep in mind, too, such possibilities as this: Two persons in a delicate performance have very different favoring conditions. For example, some people think best, they say, with the radio turned on; some people think best with it off. (You can think of better illustrations than that.) Suppose our actual experimental conditions happened to divide

subjects in the respect of favoring and hindering them, and we pooled the group of data for evaluation. It would not be striking since some of the subjects might have had negative deviations by reason of these conditions and the two tendencies cancel each other. Or take a single individual; on some days things will make him favorably inclined toward good performance—events of the day, the atmosphere of the laboratory, and so on. The conditions of the next day may be the reverse, and scores be affected accordingly. Or changes may occur in the course of the sitting itself within the hour and conceivably even in the run of 25 calls. We need to find ways of measuring the data delicately enough to catch any such effects.

Dr. Greville:

Dr. Rhine, there is one other point at the end of Dr. Lemmon's paper. He speaks about the importance of avoiding selection and he says the easiest way practically to accomplish this would seem to be to determine in advance of each experiment just how many guesses are to be included (as nearly every critic on this topic has suggested.) He says further that even this method is not entirely free from error. For example, the experimenter may decide to stop at advantageous points in the day's series, thus ending each day with a favorable balance, yet still keeping within the set total for the series.

Since Dr. Lemmon is not here today, perhaps it is unfair to go into this. Doubtless what he means is that we might make the total deviation of the entire series greater or less by merely lengthening or shortening the sections in which this series is carried out. The fallacy of this was pointed out earlier this afternoon. There is no basis for supposing that because a subject has been doing well by chance alone that he will do as well on the next run made immediately instead of holding over to a later period. This is an old idea that often has been expressed (but does not hold water), namely: If you toss a coin and get heads nine times, that makes it more likely you will get heads another time. But if nothing but chance is operating, we have no reason whatever to suppose that because the first fifty runs were good, the next fifty are going to be good, just because he is going to do the next fifty right now instead of waiting until tomorrow. The only hypothesis on which we could make such an assumption would be that the subject has the same capacity for making the second run good that he had for the first one. There is nothing in chance in the shuffle of a deck of cards that is going to do it.

Dr. Miner:

If the subject has the idea whether he is going to be good or bad, fatigue or various factors make a difference, so he might tell before you even did the first series.

Dr. Greville:

If it is just chance operating, fatigue and those other factors would have no effect on that.

Dr. Calvin (Yale University):

I wanted to ask whether you are justified in assuming in a repeated series of this sort that chance alone might be operating or is that what you are trying to disprove?

Dr. Greville:

What ESP investigators are trying to show is that chance is not the only factor involved nor the chief factor involved. So we are making the hypothesis that chance is the chief factor and then show that that leads to inconsistencies; therefore it is necessary to postulate some other factor.

Dr. Rhine:

I wonder that no one has pitched into the final proposition made by Dr. Greville in his paper that we can practically disregard this section in here (indicating on diagram the remainder of the data of a series after the most favorable stopping point). What Dr. Greenwood and he are trying to get at is a safe limit which will enable us, let us say for any purpose of evaluation of the data, to move up the gauge of measurement with entire safety into whatever area general inspection would indicate might be of interest. You will notice what they have done is to raise the critical ratio required for significance; but the method may be quite useful even so. Dr. Greenwood's measure is getting at another type of problem, the type of thing which Dr. Pratt and Mr. Woodruff were dealing with. This deals with the adding on of runs by the block setting up, say, 300 runs per block and after that adding another 300 as long as desired. Dr. Greenwood has an unpublished method of making the correction necessary for optional stopping in blocks.

Dr. Greenwood:

I want to make it clear that the two methods measure similar situations. The difference is that Dr. Greville treats blocks of one run as contrasted with my blocks of several runs each.

Dr. Rhine:

Yes, but the problems are different for us. It is different on the one hand to be able to go back over a long series and pick out a section for separate evaluations and, on the other, to be able to add on blocks of data as long as we want to.

Dr. Greenwood:

Both have their disadvantages. You might, in my case, get a pretty good approximation by not having so many stopping points; and in Dr. Greville's case, with not as close an approximation, he could have the advantage of many more stopping places.

Dr. Greville:

My method is perhaps a little more flexible from an experimental standpoint. It raises the critical ratio perhaps too much. I can't say just what

the probability is. I can say it is less than a certain figure, and that figure may be higher than you like. Dr. Greenwood's method is not as flexible in use, but it doesn't raise the ante so much, you might say. For instance, if you use my method, this data that Pratt has put on the board, I couldn't declare with confidence on that basis that it would be significant at all because my method, instead of giving an accurate estimate of the probabilities, just says I know it is less than some figure which may be much too large a figure.

Dr. Rhine:

This measure here would probably be a significant one even by your criteria, would it not (5.0)?

Dr. Greville:

It probably would, but I can't be sure of that.

Dr. Rhine:

You can see, I think, that those of us engaged in this research are eager to get all possible assistance from you and I don't want to suggest dismissal as long as there is likely to be further constructive comment. (Pause) Thank you very much.

LETTERS AND NOTES

To the Editors:

The discovery of a new and astounding set of scientific facts is inevitably followed by a wave of overenthusiasm. Overenthusiasm results in undesirable publicity, commercial exploitation, incompetent experimentation, and exaggerated claims. In general, scientists are men of high repute and character, but they are also human. Psychology has much to learn from the older science of medicine. The history of medicine is replete with sensational discoveries, universal panaceas and revolutionary cure-alls, most of which have been discredited after subsequent study demonstrated them to be ill-advised or fraudulent. The motivations for making premature or fraudulent claims are usually obvious. The discoverer gains transient attention, repute or material rewards. Medical men are no different from psychologists or parapsychologists. Psychology is too young a science to have gained bitter experience with fraudulent or ill-advised claims, but the numbers of such claims will undoubtedly increase if it becomes profitable to make them.

Organized medicine has established an elaborate system for verifying the facts concerning any particular claim or set of data. Experiments are repeated in many different laboratories. Unusual clinical cases or specimens are exchanged between different hospitals or universities. The American Medical Association has appointed special committees to act as referees on certain types of claims. Unusual claims are not accepted unless accompanied by irrefutable proof, or unless they are subject to subsequent verification.

Parapsychology is particularly vulnerable to suspicion and doubt from other professional groups. As uncontrolled experimental factors have been progressively analysed, the average number of hits in long series of runs has steadily diminished. Only a few isolated subjects have obtained extra-chance ESP scores in long runs. Unfortunately, these remarkable subjects have not been sent around to neutral laboratories for retesting. Nor have they been studied over a long enough time to determine the constancy of their abilities. There has been too great a tendency to dwell on one set of results, such as the Riess series, which cannot be verified by retesting.

It cannot be too strongly emphasized that a remarkable subject who consistently scores far above chance expectations is such a scientifically important and rare case as to warrant the most careful study by neutral referees. Future experimenters should be obligated immediately to announce their discovery of a remarkable subject so that neutral referees could vouch for the validity of the claims. In the event that other remarkable subjects are discovered, it would be worthwhile for the American Psychological Association or other interested group to set aside funds thoroughly to investigate each experiment. These comments are not intended to cast suspicion on any set of experimenters or data. They merely attempt to state the most common criticism directed against ESP experiments by scientists in related professional groups.

Finally, it would seem important to allow a gifted ESP subject the opportunity to make predictions which might result in his economic gain. It would indeed be remarkable if ability in extra-sensory perception is restricted to ESP card predictions. The ability might be applied to games of chance, or the prediction of economic trends, where material gain would result from extra-chance predictions.

F. C. THORNE, M.D., Ph.D.

June 3, 1939

Professor Gardner Murphy
Department of Psychology
Columbia University
New York, N. Y.

Dear Prof. Murphy:

The members of our committee have criticized the Murphy and Taves manuscript, "Covariance Methods in the Comparison of Extra-Sensory Tasks." Joint communications have been prepared by Drs. Hilgard and Kennedy, and by Drs. Dick, Long, and Sells. Individual reviews were submitted by Drs. Gibson, Lorge, Willoughby, and Volkmann.

Inasmuch as most of the specific points raised in relation to the original manuscript, such as ambiguities in the labelling of tables and charts, and in the identification of specific data, have been accepted and corrected prior to publication, we shall summarize in this letter only the more important methodological criticisms raised by various members of the committee:

1. The description of experimental procedures appears sufficiently detailed and complete to permit duplication of the experiment by another laboratory.

2. No questions concerning faulty experimental technique have been raised by the committee. However, a number of points concerning treatment and interpretation of results have been made.

3. The principal criticism concerning treatment of data was that adequate controls of possible linkage among target decks were not shown in the original manuscript. The members of the committee requested such controls in order to determine whether or not the reported covariance results for calls were spurious. Hilgard and Kennedy; Dick, Long, and Sells; Willoughby; and Lorge requested specific controls of these factors.*

4. The original manuscript based the proof of the covariance hypothesis upon the concomitant variation of playing-card (color) scores and circle-cross and circle-blank scores as dependent variables. Dick, Long, and Sells; Willoughby; and Lorge specifically suggested complete rotation of all variables, playing-cards (color and suit), ESP, circle-cross, and circle-blank as dependent and independent variables.**

SAUL B. SELLS.

* The additional controls of linkage, requested in item No. 3, appear on pp. 62-65.

** The intercorrelations requested in item No. 4 appear on page 57.